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ART. VIII.—*The Flood of the Connecticut River Valley from the melting of the Quaternary Glacier;* by J. D. DANA.

THE explanations offered in my last paper * with reference to the origin of the so-called "kames" in the Connecticut Valley, and of terrace-formations in general, suppose certain conditions and results that are fundamentally at variance with other views of Mr. Upham. It is necessary, therefore, to consider the facts bearing on these points of difference in order to clear the subject, if possible, of the doubts with which in some minds it is at present beset. These points involve the question as to the relations between the position, features and heights of the valley-deposits and the depth, pitch and velocity of the flooded river. The latter subjects are consequently among those that are discussed in the following pages. As the Connecticut was one of many flooded streams in Glacial America, the conclusions here deduced, if well established, have an importance as universal as the floods. This difference is however to be noted between New England rivers and many to the west or south of west, that the former were wholly within the glacier-covered region.

In the following pages I, first, briefly state what appears to have been the general condition of the Connecticut and its tributaries during the progress of the flood; and then consider: (2) the origin of the channel-way of the river; (3) the question as to which is the *normal upper* terrace in any part of the

* This Journal, xxii, 451, 1881.

valley, or that which most correctly registers the highest flood level; and next, take up the subject (4) of the dimensions, velocity and discharge of the flooded river, and (5) the bearing of the facts on the retreat of the glacier.

1. *General conditions of the flooded rivers and tributaries during the melting of the glacier.*

1. *Like other river-floods.*—The view to which I have been led by the study of the phenomena of the Champlain period, or those of the melting glacier, is that a *river-flood then* was in most points like a river-flood now. The valleys were, at the outset, deep valleys, and generally of greater depth than at present; for many have thick deposits over the bottom that were left there by the flood.

The waters of the Connecticut, as well as of other streams, rose gradually; passed the ordinary modern limit of 20 to 30 feet; still kept on rising for a prolonged period, owing to the long continuation of the melting and the abundant rains of that eminently *pluvial* era; and, at last, they reached a height of 200 to 260 feet above modern low water in the river. During its progress and continuance, water, bearing sand and often gravel and stones, flowed down the slopes leading into the valley along all the smaller depressions of the surface as well as by the many tributary valleys. Abrasion, transportation and deposition, the ordinary agencies in river action, went forward, as usual, together; and so the waters kept a great channel open along the course of greatest velocity, like a modern river, and transported and deposited according to the supply and rate of movement.

Whenever, finally, the diminution in the supply of water from the melting glacier failed of compensation by an increase in the rains, the decline of the flood began; and, as the river retreated toward its present contracted condition, it dug down into the bottom deposits to make its new or modern water-channel and flood-grounds.

2. *Changes in tributaries with the progress of the flood.*—The waters rose concurrently in the main river and in its tributaries; all had augmented activity together. At first, little streams and large ones may have been alike great transporters of sand and gravel. But with increasing height of flood there would have been changes. Some of the fierce little torrents would soon have become quiet through submergence by the great river. Other longer ones, flowing in rapids from the remoter hills, would have had their embouchures set back, but notwithstanding would have continued their transportation of coarse gravel and finer material to the end, making most abundant and coarsest beds when the flood was at maximum

height and violence; and the deposits now at or about their mouths contain, at various levels, such evidences of their perpetual activity. Others larger still, that now flow in broadly expanded valleys, would have lost, through the flood, their former pitch and torrential character, and have become for the last few miles comparatively feeble in transportation, capable of carrying and depositing only the finer materials: although farther up stream they might have continued to work as before. White River was an example. Such a large stream might however have made coarse deposits at its mouth on the borders of the Connecticut, because of the concentration carried on by the drifting action of the current of the main river, and through contributions from ice-floes coming down the main river or its tributary.

3. *The till the chief source of transported materials.*—The tributary streams as well as the main river would have derived the chief part of their material for transport from the unstratified drift or till. For the glacier had already deposited it, or the larger part, and it lay over the wide surface of the country open to receive the flowing waters, and ready for easy removal by them.

The *western* tributaries, Wells River, White River and the rest, consequently, did not gather for transportation chiefly material from the Green Mountain rocks or those of other elevations about their sources, but the universal northern drift. Hence, the drift or till west of the river, while mainly from the northwest, came partly also—especially that over the western borders of the valley—from the region of the river and its head-affluents to the north and east of north; for the glacial scratches indicate movement of the ice of the valley in the general direction of the valley. Consequently, eastern and western tributaries and Connecticut river ice-floes may have carried in stones from the same northern and north-northeastern source.

Moreover, since the till consists ordinarily: of (1) stones, large and small, (2) earth or finely pulverized rock, and usually (3) of more or less clay, it supplied (*a*) clay for clay-beds, (*b*) sand for sand-beds, and (*c*) gravel and stones for coarser deposits. And, as the rising waters reached successively higher and higher till-covered levels, submerging ledges and the lower hills, and rising against the slopes of the higher, so clay-beds should have been formed from the clay of the till at various levels in the terrace formations, like the sand-beds and gravel beds;* but the extent of them should be relatively small, or in proportion roughly to the clay in the till. The material of clay-beds may have come also from other sources;

* The heights of many of the clay beds are mentioned beyond.

but there is a remarkable uniformity of characters in the clay beds, especially the higher, which seems to indicate that they had in general the same glacial source.

4. *Deposition. Terraces.*—As happens now, deposition should have taken place over the flood-grounds, either side of the channel, wherever the waters were more or less retarded, and over the bottom according as the velocity there allowed of it. Through these depositions, the terrace-formation and its terraces were made. Along the side of the swift-flowing stream, one or both, where coarse material was contributed by tributaries, the depositions may often have been of the coarsest kind, while 100 yards away they were of sand alone, as numerous examples illustrate.

With the increasing height of the waters, the flood-grounds would have received additions in height through new depositions. But this increase would have been *small, unless* the amount of material supplied for deposition were under constant augmentation so as to have kept pace with the increasing width of the stream, and most markedly so if for any long distance tributaries were absent or feeble carriers; and small, also, if the rise of the flood went on too rapidly to allow time for the slow work of transportation and deposition. The conditions for progress in the different parts of a valley would thus have been very diverse; and the diversity under them in the resulting height would have been greatly enhanced by variations as to breadth and depth, or receiving capacity, in the valley itself.

But meteorological phenomena have their long cycles; and these would have made the progress of the flood intermittent, causing times of slowly augmenting depth to alternate at long intervals with periods of nearly stationary level; and this circumstance might have afforded a chance for approximate uniformity of height in terrace-plains at certain levels. Such a long period of nearly stationary level might have given time for building up the deposits to a common height, that of the existing water level, under different rates of supply of material, over very wide regions. A greater height than the water-level is not generally produced in such cases because of the chiseling or abrading action carried on by the surface portion of the waters, and the fact that the surface is the upward limit of deposition.

In this way a terrace of nearly uniform height could have resulted; and, during another such intermission, another; and so on. Two prominent ranges of terrace-plains may be distinguished between the level of the modern flood-ground and that of maximum flood, one at 60 to 80 feet above modern low water and another 40 to 60 feet below the maximum.

According to the process described *the terrace-plains were*

formed during the rise of the waters. This, though contrary to the views ordinarily held, and to an early opinion of the writer, appears to be true for the larger part. Yet many variations in the levels were no doubt produced by the abrasions and depositions attending the decline of the flood.

An argument against the making of the terraces of the valley by abrasion, forced on my attention through all my examination of the Connecticut Valley the past season, is afforded by the character of those plains throughout the valley. Except in the vicinity of tributaries, the lower terrace-plains consist almost uniformly of fine sand or sandy loam or clay, like the deposits beneath. Had the plains resulted through the removal of overlying deposits, that is, the grading down of higher terraces to the level of the lower, the action of the abrading waters would have generally left behind much gravel over the surface. Nothing like this ordinarily exists over the lower terraces.

Such a gravelly surface is however oftentimes found over the upper terraces, or within 50 feet of the highest terrace-level, partly as a consequence of the increased velocity of the river when the flood was approaching its maximum height, but to some extent, also, as a result of abrasion, which has resulted in depressing the surface and occasionally in making what look like broad channel-ways over it. It is of importance to note that such abrasions over the terraces by the waters flowing above them are wholly different in kind from the channelling and gouging done subsequently after the flood had subsided.

5. *The era of the glacier over the Connecticut Valley and of its melting continuous.*—Along the southern glacier limit in North America west of New England, interglacial epochs subordinate to the general Glacial period have been recognized. In the Connecticut Valley region, however, I have found nothing to indicate any such variations in the ice. The southern end of the valley, on the Sound, was at least 30 miles north of the southern line of the glacier, and hence the region was wholly within that of thick ice; and this is probably the reason for the absence of such evidence. While the facts about the White Mountains prove the ice to have had a thickness of at least 5,000 feet, the position of the New Haven region on the Sound favors the idea of a thickness not less than 750 feet. The situation of the valley with reference to the glacier, and the hilly features of the adjoining country hence explain why the valley deposits should make a continuous series from the lowest to the highest.*

* The Champlain period is, as regards the glacier, a part of the Glacial period. But in view of the fact that a great physical change took place in the relations of the land and sea (as proved beyond), initiating the era of melting, it stands apart from the true Glacial period.

This last remark has no reference to the region of the White Mountains, where the ice may have been long continued, with the various phases of modern glacier-covered mountains, as urged by Professor C. H. Hitchcock in the first volume of the Report on the Geology of New Hampshire. Yet some of the transporting effects attributed by him to such local glaciers may, I think, be due to transportation by floating ice.

2. *A Channel-way a consequence of the flow in the flooded river.*

A channel-way exists as a consequence of the flow in all modern rivers. But, according to Mr. Upham, in his Geological Report on New Hampshire, the filling up of the Connecticut valley with stratified drift took place in the era of the melting, or the Champlain period, and the excavation of the channel and the making of the terraces, in a later era. He says, p. 15:

The material was "brought down by the glacial rivers from the melting ice sheet, or washed from the till after the ice had retreated;" and by this means "the open valleys became gradually filled with great depths of horizontally stratified gravel, sand and clay." "The modified drift thus increased in depth in the principal valleys through a long period, which may have continued until the last of the ice at the head of the valley and of its tributaries had disappeared." After this came the "Terrace Period," and during it, as the sentence following the last cited observes, "the rivers have been at work excavating deep and wide channels in this alluvium. The terraces mark heights at which, in this work of erosion, they have left portions of their successive flood plains." "In this way the Connecticut River, along the greater part of its course on the west border of New Hampshire, has excavated its ancient high flood plain of the Champlain period to a depth of from 150 to 200 feet for a width varying from one-eighth of a mile to one mile, leaving numerous terraces at each side."

On page 59, he says: "The formation of the terraces has taken place by excavation of a vast deposit that filled the valley level with these upper plains." On page 44: "When the river entered upon the work of excavating its present channel in the alluvium, the kame was a barrier which confined erosion to the area on one of its sides and protected its opposite side."

This hypothesis makes two great floods necessary to the results, one for transportation and deposition, and another for abrasion, when, in fact, one may have done both. In an ordinary river, the powers of transportation and abrasion are combined in their action; and a channel between flood-grounds exists because the transporting power is lessened on either side by loss of velocity. The channel and its alluvial borders thus take shape together—the former through the latter.

To make the depositions extend up to a common level across a valley, or to approximate to this condition, either (1) the valley must have no outlet and the water therefore no current or velocity, so that contributions from the bordering slopes shall deposit equally, as nearly as possible, over the general area; or (2) the amount of transported material must be so large that transportation and friction use up the working force of the stream and reduce its velocity almost to zero.

Neither of these conditions was a fact in the flooded Connecticut. This is manifest from the varying nature of the deposits; and from the oblique lamination and flow-and-plunge structure characterizing the bedding in many parts of the valley. It is equally evident from the fine straticulation of the sand-beds. Had the stream been so clogged by its transported material as nearly to lose its motion, the assorting and stratifying action of flowing water would not have been exhibited so universally, and especially not in producing so generally a fine straticulate bedding of the sands and clays, the little layers of the beds but a fraction of an inch thick. This structure, like the same in the alluvium of the Nile, is proof of deposition by flowing waters, for each little layer required a separate special act in the flowing movement, or else successive conditions of supply and deficiency such as come by the changes of the day, or season, or year. From one end of the valley to the other there is this evidence in favor of the Connecticut's having been, through all the flood, a free-flowing stream, very much as now.

We are therefore safe in concluding that in the Connecticut valley the great stream which deposited the sands and gravel flowed with considerable velocity; and, if so, the making of a channel would have gone forward with the making of the depositions, the current where of greatest velocity locating the channel, and retardation either side by friction along with the attendant conditions of eddies locating the depositions.

Further, as is illustrated in all modern streams, a channel-way should have steep sides. The present pitch of the terrace-fronts toward the river, 35° to 42°, corresponds to the angle of rest in sliding gravel or sand when dry or nearly so. However steep the fronts at their formation, the retreat of the waters at any time would have left them to this treatment of gravity, whether formed as the flood rose, or by abrasion along the riverward margin of the terraces while it was declining.

The channel-way being thus the result of the dynamics of flowing water, no ice was needed along it, or "kame" ridges on its sides, to fix its shape or limits.

The extensive terrace-formations of the Sacramento River valley, in California, among the first studied by the writer (a

little over forty years ago),* were deposits far away from the region of ice; for the glacier of the Sierra Nevada, according to Whitney, and also Brewer, did not reach down within 2,000 feet of their level. Yet they have nearly identical features with those of the Connecticut. This is true even to the greater coarseness, in many parts, of the upper deposits. The route of the party I was with, after reaching the valley of the Upper Sacramento, just south of latitude 40° 45′, led us for fifteen miles over a high terrace-plain covered thickly with stones one to three inches in diameter, much to the annoyance of our already jaded horses. The channel-way, or open space between the high terraces of the east and west sides of the river, the terrace-plains, the terrace-front of 40° and sometimes 90° through undermining abrasion, were peculiar only in their wonderful extent.

The *width* of the channel-way has other determining causes besides velocity: such as the amount of transported material for deposition, and the form of the enclosing valley, as already explained. Along the Hudson River below the Highlands, the waters at the time of the great flood were 75 to 95 feet above the present level; and yet, owing to the nearness of the rocky hills, the channel-way was not a fourth wider than the present river; for the higher terraces rise almost immediately from its banks. On the other hand, the channel-way of the Connecticut at Hartford, Conn., had a width of 12 miles; owing partly to the low level of the region, and largely to the long trap-range on the west, shutting out tributaries and their contributions of sand and gravel. The Farmington River which comes from that direction through a cut in the trap-range, and reaches the Connecticut half a dozen miles north of Hartford, left a large part of its transported material west of the range in the Farmington Valley, where it made terraces over a hundred feet in height.

3. *Normal upper terraces.*

Professor Edward Hitchcock, finding that the terraces of the Connecticut about the mouths of tributaries were much the highest, designated them, in his "Surface Geology," "delta-terraces;" and Mr. Upham, adopting the view, represents them as terraces above the level of the "highest normal terraces" of the valley. The difference in height between the two kinds of terraces, the "highest normal terraces," and the "delta-terraces," along some parts of the Connecticut, are stated by Mr. Upham to be 40 to 50 and sometimes 100 feet.

* October 11, 1841, on a land expedition from the Columbia River to San Francisco, connected with the U. S. Exploring Expedition under Captain Wilkes. See my Geological Report of the Expedition, 756 pp. 4to, 1849, pp. 659–674; and for an abstract, this Journal, II, vii, 257, 1849.

I cite here Mr. Upham's explanation of the delta-terraces and their relation to the "highest normal terraces," from page 16 of his Report. "Upon entering the large valleys, tributary streams of comparatively narrow channel and rapid descent frequently formed extensive deposits in the Champlain period, similar in material to the flood-plain of the main valley, but having a greater height. Sometimes these *deltas*, being partially undermined, form conspicuous terraces a hundred feet above the *highest normal terrace*, which is the remnant of the river's continuous flood-plain. The deposition of the modified drift of the main river was usually, not always, to the same level across the valley. The increased supply from tributaries was sometimes a temporary barrier, damming up the waters of the main valley above; and the current could then deposit its sediment principally upon one side, making the highest normal terrace quite different in elevation."

To understand this explanation, Mr. Upham's idea as to the filling of the valley with deposits before the making of the channel-way, considered on the preceding page, must be in mind. If the making of the channel-way and terraces went on together, as appears to have been the case, some other conclusion, both as to the "delta-terraces" and the "highest normal terraces," must be found. The terraces of the latter kind, in any part of the valley, are rightly the highest there observed by Mr. Upham, with the exception of the "delta-terraces;" but if the conditions in the drainage area of the Connecticut valley were those of a flooded river with simultaneously flooded tributaries, the so-called "delta terraces," or terraces at the mouths of tributaries, may mark the height of maximum flood in the main river. The ramifying streams would have constituted a continuous range of waters, like a river and its tributaries at the present time, and would have made a continuous system of deposits. The large tributary would have entered the Connecticut with no greater pitch perhaps than that of the Connecticut itself; while smaller streams might have had a somewhat higher pitch according to the slope of the surface beneath, or the amount of sand and gravel with which the waters were loaded; a pitch of *ten* feet a mile under the last-mentioned condition would be large, of *fifteen*, very large, and of *twenty* feet, improbable. Only in case a tributary had ridges about its mouth so as to favor the formation of a dam, or had had its way obstructed by a barrier of ice, would terraces of abnormal height and pitch have been made. Otherwise the waters of the Connecticut would have extended into the mouth of the tributary, the one blending with other. The terraces of the latter, the so-called "delta-terraces," might thus have been part of the terrace formations of the main river. And such "delta-ter-

races," especially those that project out into the Connecticut valley so as to terrace it for some distance, may be and generally are, the best indicators of maximum flood-level to be found, the true highest normal terraces; because, as already explained, the supply of detritus for terrace-making was there the greatest. Were the waters so clogged by the material under transportation as to flow like mud, the angle of pitch might be even as high as 15 or 20 degrees. But the stratified and straticulate structure of the deposits proves that such a condition was of rare occurrence.

From this obvious relation of the tributary terraces to those of the main river it became of the highest importance to ascertain whether the "delta-terraces" of the different tributaries might be safely taken or not as the highest normal Connecticut terraces for the part of the valley in which they occur, and the study of this point was one of the prominent objects before me in my examination of the Connecticut valley. I found, in nearly every case investigated, that the outer part of the tributary terraces would have been within the range of the flood waters of the Connecticut; so that, while the tributary was pouring in its abundant detritus, the main river would have been distributing and leveling it. This was so with Oliverian Brook in Haverhill, where it opened on the Connecticut. The normal terrace of the Connecticut, at the mouth of this stream, is stated by Upham to be only 83 feet above low water in the river, while this brook opens on the valley with a terrace over 200 feet in height. That this brook should have held its waters up to this great height and made a terrace at that level, when those of the Connecticut were more than 100 feet below, is not possible, unless the side of the Connecticut against the mouth of the stream were blocked with solid ice: and this condition is shown by other facts not to have existed. The facts are similar in the case of the "delta-terrace" of Israel's Brook in Lancaster; of Jacob's Brook in Orford, five miles north of Thetford; of Eastman's Brook in Piermont; and of most other delta-terraces.

The true *normal upper* terraces of a valley are *those that register the same high flood-level.* They are not always easy to be distinguished, because of the inequalities in height from unequal deposition or subsequent abrasion; but with careful comparisons good general results can be reached.

The range of *normal* upper terraces along a valley should have a pitch down stream corresponding in a general way with that which the river had at the time of the flood. Mr. Upham's Report states that the upper terrace of the Connecticut valley becomes *suddenly lower* above Thetford, where the long "kame" ends; is reduced at Ely, two miles above Thetford, from a

height above low water in the river of 140 feet to that of only 55 feet; that it has this low level, continued northward, with little change, for eighteen miles, or more than half way to the mouth of the Passumpsic; and that at Haverhill, seventeen miles north of Thetford, the "highest normal terrace" is 83 feet above the river, while twelve miles to the north it is 135 feet. But, taking the terraces at the mouths of tributaries as marking the flood-level along this part of the valley—the so-called "delta-terraces" of Oliverian Brook, Jacob's Brook, Eastman's Brook and others—and then the low terraces referred to become simply the lower terraces along a part of the valley in which the highest normal terraces are wanting, except at the mouths of tributaries.*

With regard to the high terrace-plain of Haverhill there are other facts besides those connected with the terraces of Oliverian Brook; and these are presented on a following page.

[To be continued.]

ART. IX.—*Geology of the Diamond;* by O. A. DERBY.

TWO papers on this subject have lately appeared in Brazil in the Portuguese language. One by Professor H. Gorceix, of which an abstract was given in the September number of this Journal, treats only incidentally of the diamond. Its conclusions are, that the diamond, like the topaz, originated in the series of granular quartzites (itacolumites) and unctuous schists, which is so largely developed in Minas Geraes, and that the itacolumite is possibly the original matrix of the gem.

In a paper by myself, published in vol. v of the Archivos do Museu Nacional of Rio de Janeiro, the famous locality of Grão Mogol, in which diamonds are found in quartzite, is discussed. It is shown that, under the name of itacolumite, two very distinct geological series have been confounded. The older series, including the true flaggy and often flexible quartzite to which the name should be limited, is intercalated with the unctuous (hydro-mica) schists and itabirites. The newer series is composed almost exclusively of quartzite, which in its finer parts is almost indistinguishable from true itacolumite, but which, in places, passes to a conglomerate containing pebbles of all the rocks of the older series. Throughout the diamond region of the Serra do Espinhaco this quartzite lies in well-marked unconformability on the upturned edges of the lower, though, since

* Mr. Upham explains, (p. 79,) the existence of terraces of small height as the "highest normal terraces," in some places low while high elsewhere, by assuming "an irregular rate of retreat of the ice-sheet, allowing long and abundant deposition in some portions, but much less in other portions of the valley."

the localities where the two quartzites can be seen in juxtaposition and where they are at the same time clearly distinguishable one from the other are few, this unconformability has escaped notice or has only been doubtfully recorded. The distinction between the two series of quartzite being established, it is shown that the diamond-bearing rock of Grão Mogol most probably belongs to the newer series, and that the diamond entered into its composition ready-formed like any other pebble. The locality of São João da Chapada, where the diamond has been mined in clay (*barro*), is described. It is shown that the mine is excavated in the undisturbed soft material resulting from the decomposition of beds of unctuous schists underlying a bed of quartzite (itacolumite) which appears at the entrance of the mine. The diamond-bearing material was not exposed *in situ*, but two masses dislocated by slides were pointed out by a negro, who knew the mine thoroughly, as the diamantiferous *barro*. One consisted of a black clayey mass which, on a fresh fracture, revealed thin alternating layers of white clay resembling lithomarge and black pulverescent iron oxide. The other mass consisted of a portion of a quartz vein, the quartz being much fractured and traversed by brilliant plates of specular iron, with a mass of decomposed shale adhering to one side, and with an undisturbed mass of red clay adhering to the other side, to which in turn was attached a mass of decomposed shale. The vein nature of this mass was unmistakable. The red clay, which was said to be the diamond-bearing material, is rich in iron, and on treatment with acids leaves white quartz sand and an abundant black sand, consisting for the most part of microscopic tourmalines, according to the determination of Professor J. W. Mallet. Small hexagonal crystals are also described by H. Rose in a mass of *barro* containing a diamond obtained at São João by Messrs. Heusser and Claraz. It was concluded from these observations that the diamond occurs at São João in its original matrix, and that this matrix is a vein of quartz accompanied by a rock of unknown nature, but containing iron and tourmalines traversing the series of unctuous schists and itacolumites.

Since the publication of these papers, both Professor Gorceix and myself have revisited the diamond region, and these views have been fully confirmed. A specimen of rock from Grão Mogol, obtained through the kindness of Dr. Cartão Jardim, shows distinctly by the side of a diamond a rounded water-worn pebble, and Professor Gorceix was so fortunate as to extract, under his own immediate supervision and with all necessary precautions, several diamonds from the *barro* of the São João mine.

Near Diamantina I examined a mine in a rotten conglomer-

ate, which I suppose to belong to the same series as that of Grão Mogol. At other points, near the São Francisco river, diamonds appear in a region of a newer (though probably Paleozoic) conglomerate, and in the province of Paraná, in a region of Devonian sandstone and conglomerate. In all these cases the diamond has most probably come out from its secondary deposit—the conglomerate. Of course all rocks newer than the original formation and formed from its debris *may* contain the diamond. The original formation is most probably of Cambrian age.

ART. X.—*On the Algebraic Expression of the Diurnal Variation of Temperature;* by B. A. GOULD.*

ALTHOUGH the general principles, which I am about to consider, apply to the cyclical mean variations of all the meteorological elements, we will restrict ourselves at present to the question of the diurnal changes of temperature; not merely for greater brevity, and because these fluctuations have a wider range and are relatively less affected by abnormal influences than those of the atmospheric pressure, moisture, etc., but because they have been made the subject of more extensive investigation, and because it is more especially regarding these that serious and harmful misapprehensions appear to exist.

It can scarcely be questioned by any one, who will give the subject his unbiased reflection, that a phenomenon due to the rotation and revolution of the earth,—however its direct manifestations may be modified by indirect and collateral influences, also purely natural,—must be governed by general laws which cannot fail to become manifest in the mean of observations sufficiently numerous. For if the indirect perturbations of the fundamental law belong to the class which may properly be regarded as casual, they will be eliminated from the mean values; and if they are not of this class they must and should be included in the general law which it is desired to discover.

Nor can it be reasonably denied, that if the diurnal variation be in fact governed by a general law, this must be of a cyclical nature, and capable of expression by some formula which completes its period with the solar day.

There are various formulas of this class, many of them interconvertible by simple analytic modifications; but none, of a thoroughly general character, which is simpler than a series progressing according to sines and cosines of successive multiples of the time elapsed since an adopted epoch. For phenom-

* Translated from Vol. II. Chapter vi, of the "Anales de la Oficina Meteorologica Argentina," by the author.

ena of the class in question, where the curve is continuous, this is of absolute generality; and, although other forms of expression may claim equal accuracy, none surpasses, or probably equals, it in the ease with which its constants may be computed, or the facility with which its value may be deduced for any value of the variable. It is unlikely that these advantages would escape the attention of any mathematician who might desire to give algebraic expression to the law of a periodic phenomenon. But it is generally known among meteorologists by the name of Bessel's formula, not so much because it was first employed by that great astronomer in studying the diurnal oscillations of the barometer, as on account of the elaborate articles in which he long afterward pointed out its generality, its convergence, and the special circumstances which facilitate its employment and application; and earnestly commended its employment to meteorologists in determining the diurnal and annual variations of atmospheric phenomena, the influence of winds from different directions, etc.

In fact a general algebraic formula gives, as Bessel said, the result of observations expressed in the concisest form. To translate his words: "by deducing a formula which represents a phenomenon, we meet its theory half-way; thus the observed longitudes of the sun, if thus expressed as functions of the annual period would have given a formula from which, were Keppler's discovery yet unmade, the elliptic motion would be far more easily recognized than from the observations directly. .... The periodic phenomena of meteorology belong to the same class, and the determination of their formulas, by means of observation, enables us to subject them to the readiest and most complete examination; so that whatever is to be explained by theory may be more directly deduced from the formulas than from the observations themselves or from the curves by which it is customary to represent the results of unknown laws."

The only true object of scientific investigation is the determination of principles and laws; and it may well be questioned what high aim can be attained by accumulating special results without such generalization.

It is therefore with deep regret, as well as surprise, that we have seen the eminent director of the Physical Central Observatory of Russia, under whose immediate supervision stand all the physical observations officially made in that vast realm, express himself in terms like the following, which I quote from among very many other statements of the same sort:

"How much time has been uselessly squandered in observations laboriously carried on through day and night, and in extended and far more useless calculations of the same by the Lambert-Bessel interpolation formula."

"In the lately published volume of the physical observations by the American expedition of the Polaris, Mr. —— has performed the most disheartening task of utterly useless calculation of all the meteorological elements according to Bessel's formula. What a pity that the time was thus lost which might have been employed in making better use of these excellent observations!"

"If then, under the erroneous idea that a certain number of terms of the Lambert-Bessel interpolation-formula can give the law of the diurnal variation of temperature, it has in many cases been sought to deduce therefrom the values for intermediate hours, or for the maxima or minima, ... we cannot wonder at the failure of such calculations."

"The application of Bessel's formula to represent the daily course of the temperature has, thus far, much more hindered than advanced our knowledge of the same."

These citations are perhaps more than sufficient for my purposes; which are: first, to show the facility with which a backward step may be taken, even by one whose services to his department of science have been and are distinguished and undeniable, when a dominant idea is allowed to distort his judgment; and secondly, to oppose the influences of argument and demonstration to these attempts at discouraging what I believe to be the only mode by which meteorology can become a science, and by which it bids fair soon to attain that rank.

Had the author perceived that the sine-formula is not simply a means of interpolation, but a concise mode of general, complete and definite expression of a cyclical law, he would have aimed his denunciations at its frequent misapplication, but not at its use. The immediate origin of his strongly expressed opinions seems to have been his recognition of a relatively sharp bend in the curve of mean temperature at many stations, near the time of sunrise, and of sundry discordances between the epochs of maxima and minima as deduced under his direction by graphical methods, on the one hand, and by computation on the other. But it would be strange if the upward bend near sunrise should not be essentially balanced by a downward flexure after sunset, although the effect of this might be less conspicuous in the curve when graphically delineated, and the sharpness with which the moments of maxima and minima may be determined depends upon the form of the curve in their vicinity. Inasmuch as the minimum usually precedes sunrise by only a small interval, while the maximum generally precedes sunset by a very considerable one, a want of symmetry in the shape of the curve near the two daily extremes ought to be expected. Using identical data, it is a remarkable assertion that the free hand and judgment of a draughtsman can afford a more correct representation of the true curve than would be

obtained from accurate numerical calculation by means of the formula.

I am not aware that it has ever been considered desirable to base extended numerical calculations upon observations made more frequently than once an hour, for the determination of the diurnal curve; nor do I understand that the employment of such an increased number is now advocated. Were this desired, the influence of the additional observations could, if perceptible, be introduced into the formula quite as well as into the graphical sketch. It may therefore be assumed that the form of the diurnal curve, as afforded by hourly observations is amply sufficient for all the present purposes of scientific inquiry.

So much being premised, it may be well briefly to consider, even though only in an elementary manner, how and to what extent the formula in question may find legitimate application.

Denoting by h the number of hours elapsed since any given moment (for which we will, as usual, adopt that of midnight), the observed temperature will be represented with absolute precision for each one of the twenty-four hours, by the formula which, in its most compact and elegant form, may be written

$$T_h = M + a\sin(h+A) + b\sin(2h+B) + c\sin(3h+C) + d\sin(4h+D) + \ldots\ldots + l\sin(12h+L)$$

in which a, A, b, B, c, C, etc., are constants which can only be determined by observation, and M is the mean of the twenty-four equidistant observations, this being the "daily mean" as defined by the established usage of physicists. It is perhaps needless to add that h should in strictness be made to denote hours of true and not of mean time; but this is a point which need not be considered if the length of the interval, for which the formula is determined, be either such as to eliminate the equation of time from the means of the daily observations, or so short that its inclusion may be recognized and allowed for, if desired.

An absolute representation of each of the twenty-four hourly observations, although afforded by the formula if its full number of twelve terms be employed,* is more than the demands of meteorology require; for all the errors of observation, not eliminated from the data, would thereby be reproduced, whereas by

* The curious numerical values upon p. 60 of Dr. Wild's "*Temperatur-Verhältnisse des Russischen Reiches*" from which it is inferred that the legitimate results from twenty-four equidistant observations cannot be fully represented by a formula containing twenty-four constants, are vitiated by the joint effect of an error of computation and the employment of an insufficient number of decimals. The effect of these errors is quite insignificant, it is true, and amounts to only a very few hundredths of a degree; but it is precisely upon these small quantities, much inferior in magnitude to the unavoidable errors of observation, that the whole argument of the author depends.

the omission of the latest terms these errors will be equated out to any desired extent in the same way that this end could be attained by graphical methods, but with far more accuracy. And a general expression, which sufficiently represents the known mean temperature at intervals of an hour throughout the day, cannot fail to give that which corresponds to any intermediate moment with all the exactness needful in the present state of science. But since each variable term of the equation as above written contains two unknown quantities, its determination demands, in the absence of other data, a knowledge of the temperature corresponding to two different hours at proper intervals.

As regards the degree of minuteness with which it is desirable that the formula should represent the original observations, there is much to say: but it will suffice in this place to call attention to the fact that the mean of values derived from a lifetime of observations would probably be inadequate to represent the true typical value, even within the limits of appreciable error. Most of the so-called casual influences, which so greatly disturb the normal values that the result of a single year's observations affords but a first approximation to the fundamental law,—are it is true, themselves subject to the law of probabilities, and are eliminated from the mean of the observations made during a large number of successive years. But in general the number of years requisite for such elimination will be far greater than is available; so that any minute and scrupulous attention to the computation of quantities smaller than the probable error of the data is misapplied, and even absurd, unless such quantities are demonstrably constant for the various individual values whose mean is taken. To devote punctilious attention to hundreths of a thermometric degree in results derived from data which are uncertain by ten or twenty times that amount is to "strain out a gnat and swallow a camel." From this point of view, also, it may be maintained that the last terms of the formula might be advantageously omitted.

The problem, which most frequently presents itself in practice, is to determine with sufficient approximation and from a relatively small number of daily observations, the most characteristic points of the curve and the daily mean. This is easy in proportion to the rapidity of convergence of the formula; and the degree of this rapidity affords a criterion of the extent to which the true law is represented by the earlier terms. If, by a few terms of the formula, containing no more than six or eight constants instead of twenty-four, the whole series of hourly temperatures can be represented within the limits of their probable errors, no further demonstration is requisite to show that the true law of daily variation may thereby be represented to an equal degree.

The general relations between the curves of daily variation at different places happily provide a means of deducing very approximate values of the constants from a smaller number of daily observations than would otherwise be requisite. Were the true moment known for either the maximum, or the minimum, or the true value of the daily mean, any one of these would supply the place of an additional daily observation. And the simple self-evident fact that, in the absence of very abnormal topographical conditions, the diurnal curve has but one point of contrary flexure, affords yet additional facilities for attaining the desired end.

The most important term of the formula, for various reasons, is the constant M, the daily mean temperature, which may be deduced with very considerable approximation from a relatively small number of observations. Thus in the mean of two values, corresponding to moments separated by an interval of half the day, all terms containing uneven multiples of h are eliminated, so that

$$\tfrac{1}{2}(T_h + T_{h+12}) = M + b \sin(2h + B) + d \sin(4h + D) + f \sin(6h + F) + \text{etc.},$$

while from the half difference of the same values we obtain the total amount of the omitted terms,

$$\tfrac{1}{2}(T_h - T_{h+12}) = a(\sin h + A) + c \sin(3h + C) + e \sin(5h + E) + \text{etc.},$$

a value which the. frequently very rapid, convergence of the series often renders extremely useful.

Thus from the mean of three equidistant values, all terms are eliminated in which the coefficient of h is not divisible by 3; so that

$$\tfrac{1}{3}(T_h + T_{h+8} + T_{h+16}) = M + c \sin(3h + C) + f \sin(6h + F) + \text{etc.}$$

Similarly the mean of four observations, at intervals of six hours, gives us

$$\tfrac{1}{4}(T_h + T_{h+6} + T_{h+12} + T_{h+18}) = M + d \sin(4h + D) + \text{etc.},$$

in which the first variable term is generally of small importance, and the remaining two absolutely insignificant.

And, in general, all those terms which do not depend upon multiples of nh are eliminated from the mean of n equidistant values so that the mean of observations made at intervals of three hours will differ from the daily mean only by the amount of the eighth term of the general formula; and that deduced from two hourly observations will differ from it only by the twelfth term. How small the coefficients of these high terms really are, will soon be seen.

From considerations analogous to the foregoing, we may easily perceive what is the amount by which the mean of determinations, made at any given hours of the day, will differ from the true daily mean. For example, if there be three daily determinations at the hours $h+m$, $h+n$, $h+p$, their mean will be

$$M + aq_1 \sin(h + A + Q_1) + bq_2 \sin(2h + B + Q_2) + cq_3 \sin(3h + C + Q_3) + \text{etc.}$$

in which

$$q_1 \sin Q_1 = \tfrac{1}{3}(\sin m + \sin n + \sin p); \quad q_2 \sin Q_2 = \tfrac{1}{3}(\sin 2m + \sin 2n + \sin 2p)$$

$$q_1 \cos Q_1 = \tfrac{1}{3}(\cos m + \cos n + \cos p); \quad q_2 \cos Q_2 = \tfrac{1}{3}(\cos 2m + \cos 2n + \cos 2p) \text{ etc.}$$

Supposing the hours of observation to be 7^h, 14^h, and 21^h, as we have arranged them for this country, the corresponding quantities become

$$q_1 = 0{\cdot}161, \quad q_4 = 0{\cdot}667; \quad Q_1 = 14^h, \quad Q_4 = 8^h$$
$$q_2 = 0{\cdot}244, \quad q_5 = 0{\cdot}311; \quad Q_2 = 16^h, \quad Q_5 = 10^h$$
$$q_3 = 0{\cdot}805, \quad q_6 = 0{\cdot}333; \quad Q_3 = 18^h, \quad Q_6 = 12^h \text{ etc.}$$

Were the hours of observation the somewhat more symmetrical, although in the present case less advantageous, ones 7^h, 13^h, and 21^h, we should have

$$q_1 = 0{\cdot}173, \quad q_4 = 0{\cdot}577; \quad Q_1 = 12^h \quad Q_4 = 6^h$$
$$q_2 = 0{\cdot}333, \quad q_5 = 0{\cdot}644; \quad Q_2 = 18^h \quad Q_5 = 12^h$$
$$q_3 = 0{\cdot}745, \quad q_6 = 0{\cdot}333; \quad Q_3 = 16^h{\cdot}77 \quad Q_6 = 6^h$$

The smallness of the coefficients for the earlier variable terms shows how near is the approach to the desired value of M. In fact the approximate value thus obtained, is usually sufficient, when considered in connection with the general form of the diurnal curve (also approximately known), to afford valuable aid in attaining a knowledge of the principal terms of the formula, as will hereafter be seen.

Let us now consider the problem in the form in which it practically offers itself for those places in the Argentine Republic at which we have been able to secure three daily observations. From these it is desired to infer the mean daily curve with the greatest degree of approximation which they can be made to yield; availing ourselves of every circumstance which can help to bring us nearer to the truth, yet avoiding illusory results which although derived from the data may have no foundation in the true law.

There are but three points in all the national territory at which more than three daily observations have been made

during a sufficient period to permit the form of the daily curve of temperature to be deduced independently. These are Buenos Ayres, where there were but two additional hours of observation; Bahia Blanca, where the different hours, though comparatively numerous, were at different dates; and Cordoba, where the series of hourly observations comprises but a small number of years. And moreover, those hours, which it has been found necessary to adopt for the regular observations made for this office in the various parts of the Argentine territory, are not those which permit an exact determination of the daily mean, even so far as the accuracy of the determination is uninfluenced by the third variable term of the general formula.

But notwithstanding these adverse circumstances, there are favorable considerations of yet more importance. In the first place, the configuration of the country is such as to make it probable that throughout the vast extent of the pampa regions, at least, the general form of the daily curve is not dissimilar; secondly, we have, in the periodic sine-formula arranged according to multiples of the time, a series of terms, each of which is independent of the rest, and susceptible of separate investigation, being in fact so many epicycles; thirdly, the mean daily curve cannot have more than one point of contrary flexure unless the local topographical conditions be so exceedingly abnormal that the investigation of the daily curve would have no general scientific value; and in addition to all these, the curvature in the vicinity of the maxima and minima is so gentle that a very considerable error as to the moment of their occurrence is productive of but comparatively slight influence upon the form of the computed curve, or upon other values which are determined through their agency.

If we neglect the variable terms beyond the third, we shall have from the mean of observations made at any of our stations at 7 A. M., 2 P. M., and 9 P. M. during a given interval, the three equations

$$(1)\quad T_{7}=M+a\sin(7^{h}+A)+b\sin(14^{h}+B)+c\sin(21^{h}+C)$$
$$(2)\quad T_{14}=M+a\sin(14^{h}+A)+b\sin(4^{h}+B)+c\sin(18^{h}+C)$$
$$(3)\quad T_{21}=M+a\sin(21^{h}+A)+b\sin(18^{h}+B)+c\sin(15^{h}+C)$$

and if the epochs H_1, H_2, of maximum and minimum together with the corresponding temperatures m_1, m_2, were known, we should have the four additional equations

$$(4)\quad 0=a\cos(H_1+A)+2b\cos(2H_1+B)+3c\cos(3H_1+C)$$
$$(5)\quad 0=a\cos(H_2+A)+2b\cos(2H_2+B)+3c\cos(3H_2+C)$$
$$(6)\quad m_1-M=a\sin(H_1+A)+b\sin(2H_1+B)+c\sin(3H_1+C)$$
$$(7)\quad m_2-M=a\sin(H_2+A)+b\sin(2H_2+B)+c\sin(3H_2+C)$$

and by the seven independent equations thus available, the seven unknown quantities which they contain could be fully determined; so that the form of the daily curve would be absolutely known, excepting so far as it might be affected by epicyclical terms which complete their period in six hours or still shorter aliquot parts of a day.

Unfortunately the data relative to the extremes are only roughly, even when at all, known; yet the condition that there shall be but one* contrary flexure in the curve, together with the possibility of inferring a very approximate value for M, from the observed temperatures at 7^h, 14^h, and 21^h, provide the means of remedying these deficiencies to a considerable extent; while it may be justifiable to assume an analogy between the form of the curve sought, and that of those already determined for Bahia Blanca, Buenos Ayres, and Cordoba, to an extent to diminish still further the probable errors of the determinations. In this way very approximate values of the constants of the first three terms may frequently be obtained; and although no one would maintain that an absolute representation of the true daily curve could thus be secured, still we should certainly obtain a curve of which the deviation from the true one is very small, and which intersects it in six nearly equidistant points.

The first, and usually the most troublesome, process to be

* Notwithstanding indications of secondary maxima and minima have been found in observations made at sundry places, and mentioned by Kämtz and Karsten, while they have been especially discussed by Hellmann, the reality of the phenomenon seems by no means proved. The most conspicuous case of their occurrence is at St. Petersburg, in the new series of observations organized by Dr. Wild with such care that he thinks the error of a single determination cannot exceed 0·1 (*Temperatur-Verhältnisse*, p. 29). In the mean of the six years' observations of this series, minima, other than those which he accepts as the true ones, occur at about $3\frac{1}{2}^h$ in November, $21\frac{1}{2}^h$ in December, and $0\frac{1}{2}^h$ in January. Although the amounts of the abnormal fluctuations are petty in themselves, they are far from being unimportant relatively. Indeed for the month of December, for which the maximum mean temperature, as given by the observations, is 7°·36 at $13^h\ 45^m$, and the minimum 8°·28 at $21^h\ 42^m$, Wild adopts as the true minimum, 8°·03 at $9^h\ 0^m$, or only thirty-three minutes earlier than the epoch which he himself gives for the mean temperature of the day.

We cannot but share his opinion that this abnormal fluctuation, by reason of which the observed temperature remains below its minimum for twelve or thirteen hours of the twenty-four, is to be "considered as a remainder from non-periodic perturbations," and likely to disappear from the mean of many years' observations. This is made probable by the fact that the twenty-two years of the old series show no indications of such a phenomenon.

The real existence of any secondary maxima and minima is flatly denied by Wild, and in this we believe him to be correct. But in accounting for their apparent occurrence, he says that it is either simply due to irregular perturbations not equated out, or *generated by calculations with Bessel's formula* (*Temp. Verhältn.*, p. 103). Had he said "erroneously inferred from inadequate data," there might have been a show of reason in his statement. But it ought to be needless to mention that errors can only be produced by the use of this general formula in the same way in which they can be caused by the use of a book of logarithms; namely by its misapplication.

effected is the determination of a sufficiently accurate value of M, the daily mean. When we know the epochs of maximum and minimum or the corresponding temperatures within narrow limits this is easy, for having once attained moderately correct values for M, m_1, and m_2, tentative processes will soon guide us to congruous values of all three which will accord with the known general form of the daily curve. This can be completely accomplished by numerical methods only, as must be the case with every process of the sort which can claim to be exact; but as is also commonly the case it may be greatly facilitated by graphical methods.

For a first approximate value of M, the mean of the temperatures observed at 7^h, 14^h and 21^h will usually suffice, although it is always too high, its value being

$$\mathrm{M}+0{\cdot}16\,a\sin(\mathrm{A}+14^h)+0{\cdot}24\,b\sin(\mathrm{B}+16^h)+0{\cdot}80\,c\sin(\mathrm{C}+18^h)+0{\cdot}67\,d\sin(\mathrm{D}+8^h)$$

but when the constants of either the first or second variable term are approximately known, a much closer estimate may be made by taking in the former case $\frac{1}{4}(\mathrm{T}_7+2\mathrm{T}_{14}+\mathrm{T}_{21})$ which gives

$$\mathrm{M}+0{\cdot}37\,a\sin(\mathrm{A}+14^h)+0{\cdot}07\,b\sin(\mathrm{B}+4^h)+0{\cdot}85\,c\sin(\mathrm{C}+18^h)+0{\cdot}72\,d\sin(\mathrm{D}+8^h)$$

or in the latter case $\frac{1}{5}(2\mathrm{T}_7+\mathrm{T}_{14}+2\mathrm{T}_{21})$ which gives

$$\mathrm{M}+0{\cdot}01\,a\sin(\mathrm{A}+2^h)+0{\cdot}49\,b\sin(\mathrm{B}+16^h)+0{\cdot}77\,c\sin(\mathrm{C}+18^h)+0{\cdot}60\,d\sin(\mathrm{D}+8^h)$$

and substituting the numerical values of the known constants. In practice it is almost always possible to estimate the magnitude of the unknown constants with sufficient correctness to deduce a very close approximation to the true values.

But, in the absence of any knowledge of the constants, a very near approach to the true daily mean may be generally obtained for this country from the combination $\frac{1}{17}(6\mathrm{T}_7+5\mathrm{T}_{14}+6\mathrm{T}_{21})$; this being equal to

$$\mathrm{M}+0{\cdot}11\,a\sin(\mathrm{A}+14^h)+0{\cdot}32\,b\sin(\mathrm{B}+16^h)+0{\cdot}79\,c\sin(\mathrm{C}+18^h)+0{\cdot}65\,d\sin(\mathrm{D}+8^h)$$

and the relations between the constants of the different terms being in this region such that the several variable terms in this formula nearly cancel one another.

With the preliminary value of M, obtained by any of these devices, is to be combined the approximate time H, of the daily maximum. This epoch, as also that of the minimum, is so variable on account of the large effect exerted upon it by disturbances of the casual class, that a very long series of years

would be needful for determining it with any accuracy. It is furthermore so uncertain, and its determination so insecure, owing to the great extent to which the time of its apparent occurrence is influenced by the inevitable errors of observation, that it may well be questioned how far the minutiæ of calculation can advance our real knowledge regarding it, and whether much time has not been uselessly expended in attempts to fix with precision a moment which is in its nature unprecise. But, without entering upon these questions, it may be stated that a considerable error in the assumed epoch will give rise to a relatively small error in the final result. Where no special means of forming an estimate can be made useful, it may be well to begin with the supposition that the highest temperature occurs at 2 P. M. daily.

If we disregard the third variable term in the equation (1) to (4), we have now a means of determining, first, approximate values for the constants a, A, b, B; and from these a corresponding curve, which must necessarily intersect the true mean daily curve in the three points given by observation. But, unless the assumed values of both M and H have been very near the truth, this very circumstance will cause the general form of the computed curve to be markedly discordant from that of the true one, already approximately known; and very frequently it will be found that double curvatures will present themselves as the only geometrical mode of satisfying the condition that the areas above and below the medial line must be equal.

A second computation is now to be made with a slight change in the assumed value of M, which a little tact on the part of the computer will usually enable him to bring nearer to the truth; but should a third be found needful, it will be convenient to change the assumption by about the same amount as before, to permit an easy use of the rule of proportion. It will be found that the increase or diminution of the values of the resultant constants depends upon the value assumed for M; and a comparison of the corresponding curves will usually permit its true value to be fixed within extremely narrow limits.

Adopting for M the value thus derived from the first assumption regarding the time of daily maximum,—the question next arises, to what extent any other assumption would bring the form of the curve into nearer analogy with any known one at some station not too diverse either in its geographical or its topographical relations. This may be determined in a similar way by varying the assumptions until a satisfactory result is attained. Should the previous value of H be found to require any considerable modification, the determination of M may be revised, although any change in this would be due to the influence of terms of the second order.

It is manifest that the trustworthiness of this method must depend upon a convergence of the successive approximations toward one and only one value of each of the desired constants, M and H. This may be tested whenever desired by a similar process, employing an assumed epoch for the daily minimum; or, what amounts essentially to the same, by comparing the epoch resulting from the determinations already made with that which is made probable by other considerations. In the case that observations of the minimum temperature exist, these will afford a more delicate criterion than those of the maximum, since the epoch of minimum is during most of the year far more distant from our hour of morning observation, than is that of maximum from 2 P. M.

It has been already stated that the earlier stages of the computation can be greatly facilitated by graphical processes, but in this place we are only considering the regular numerical routine available under all circumstances.

I append three examples, to illustrate the mode of computation. These are selected from among those places for which hourly observations are on record, because, while they are markedly diverse in their geographical relations, they also belong to the class most difficult to determine, namely that for which we can derive no indication regarding the form of the diurnal curve from a knowledge of that which corresponds to any place in the vicinity or even similarly situated. Thus we have a severe test of the degree of approximation to the true law which is attainable by means of the daily observations at 7^h, 14^h and 21^h, without any other data whatsoever. The discordances, between the hourly temperature thus inferred and those derived from actual observation, represent the sum-total of the various errors due to inaccuracies in determining the constants, and to the disregard of all terms of the general formula beyond that which depends on twice the time. It has been already mentioned that in practice the determinations may be much facilitated by the use of graphical methods for the first approximations; but we will dispense with them in our examples, and likewise with the various indirect processes which may often be advantageously employed; and begin in each case by assuming the mean of the three daily observations for the first approximation to the true daily mean, and 14^h for the hour of maximum. For readily perceiving the general form of the curve, a rough drawing is of course always desirable.

1. First, let us take the observations made in the month of January during five successive years at the island of St. Helena, and published in degrees of Réaumur's scale by Dove in his second series. Here we have $T_7=13\cdot05$, $T_{14}=16\cdot02$, $T_{21}=13\cdot74$, the mean of which is $14\cdot27$.

Putting this mean for M and 14^h for H_1, the equations (1) to (4) give $a=1{\cdot}09$, $A=225°$, $b=0{\cdot}71$, $B=41°$. The resultant curve has a strongly marked secondary minimum at 21^h with its corresponding maximum at about 1^h, showing this value of M to be inadmissible.

A second supposition, $M=14{\cdot}21$, gives $a=1{\cdot}27$, $A=227°$, $b=0{\cdot}59$, $B=44°$ and this curve also exhibits a contrary flexure analogous to the former although less pronounced.

A third supposition, $M=14{\cdot}15$, gives $a=1{\cdot}46$, $A=229°$, $b=0{\cdot}47$, $B=48°$, and since in this curve there is no secondary maximum, the question arises as to the value of M between $14{\cdot}21$ and $14{\cdot}15$ for which this disappears. It will quickly be perceived on trial that this value is very near to $14{\cdot}18$ for which $a=1{\cdot}36$, $A=228°$, $b=0{\cdot}53$, $B=46°$.

Inspecting now the corresponding curve and comparing it with straight lines drawn to connect the point for 14^h with those for the other two observations, it becomes evident at a glance that the hour of maximum was assumed too early, and that a better assumption would have been $14^h\ 30^m$. This latter gives the equation

$$T=14{\cdot}18+1{\cdot}385\sin(h+224°\ 24')+0{\cdot}508\sin(2h+26°\ 41')$$

There is in this curve no indication of any failure to fulfill all the conditions of our problem; and in the total absence of other data, the equation may be regarded as a sufficient representation of the diurnal curve. Some slightly more probable values for the constants might be obtained by new trials, which would fix better limits for M with $14^h\ 30^m$ and better limits for H_1 with $M=14{\cdot}18$; inasmuch as each of these determinations is quite close enough to render quantities of the second order negligible. Nevertheless any such additional calculations would imply a minuteness scarcely appropriate to the character of the problem. But did we possess any one of the not unfrequently available additional data which are afforded by an approximate knowledge of the time or value of the daily maximum or minimum, we should thus be enabled to fix the constants with much greater precision; while a knowledge of all of them would permit us to deduce with considerable accuracy those of an additional term in the formula.

The hourly values of the temperature which correspond to the curve just deduced are given below, side by side with those actually observed. The grouping of the algebraic signs in the residuals, and the relative magnitude of these, show how large is that part of them due to the omission of the term depending upon $2h$.

Hour.	Temperature.		O—C.	Hour.	Temperature.		O—C.
	Observed.	Computed.			Observed.	Computed.	
1h	13°·36	13°·41	−0°·05	13h	15°·85	15°·79	+0°·06
2	13·28	13·36	−0·08	14	16·02	16·02	0·00
3	13·18	13·25	−0·07	15	15·97	16·01	−0·04
4	13·15	13·12	+0·03	16	15·90	15·80	+0·10
5	13·10	13·00	+0·10	17	15·69	15·42	+0·27
6	13·06	12·96	+0·10	18	15·13	14·94	+0·19
7	13·05	13·05	0·00	19	14·48	14·47	+0·01
8	13·26	13·30	−0·04	20	13·95	14·04	−0·09
9	13·68	13·72	−0·04	21	13·74	13·74	0·00
10	14·26	14·24	+0·02	22	13·61	13·56	+0·05
11	14·83	14·83	0·00	23	13·52	13·47	+0·05
12	15·35	15·38	−0·03	24	13·45	13·44	+0·01

So far as it can be deduced from the hourly observations, the true formula to the 2d variable term inclusive (i. e. with four constants) is

$$T=14{\cdot}20+1{\cdot}415\sin(h+223^\circ\,48')+0{\cdot}500\sin(2h+20^\circ\,50')$$

and the true daily maximum is at $14^h\ 38^m$.

2. Let us next consider the observations made at Hobart Town in Tasmania during the month of January in eight successive years, also published by Dove and expressed in degrees of Réaumur. Here $T_7=11°{\cdot}90$, $T_{14}=17{\cdot}29$, $T_{21}=12{\cdot}07$, the mean of these three temperatures being 13·75.

Beginning with this for the value of M we find the secondary maximum to be very pronounced; and assuming successive values, inferior to this, we find that the highest which gives, for $H_1=14^h$, a line without contrary flexure is 13°·45, and that none above 13°·40 fails to show decided indications of a secondary maximum between 0^h and 4^h. For M=13°·40 we have $a=3{\cdot}22$, $A=238°{\cdot}46$, $b=0{\cdot}68$, $B=32°{\cdot}56$, which represent a curve with rounded and flowing outlines. Yet even this not only exhibits a very marked want of symmetry between the portions above and below the medial line, but also shows in other ways the need of further modification. For instance, its minimum occurs before $2\frac{1}{2}$ A. M., or more than three hours before sunrise, which is totally abnormal for the latitude 42°; and although the general form of the curve near the maximum,—which is fixed within comparatively narrow limits by the three fundamental observations,—is demonstrably one of rapid rise and fall, yet that portion of the computed curve which corresponds to the night hours is abnormally flat. These considerations agree in indicating that the true value of M is still smaller.

If we assume M=13°·35 we have $a=3{\cdot}37$, $A=239°\,30'$, $b=0{\cdot}59$, $B=39°\,12'$ these representing a curve which manifests the same peculiarity of a too early minimum. But by vary-

ing the epoch of the maximum, it becomes evident that the assumption of an earlier maximum will remedy this defect,—and chiefly by increasing the importance and modifying the epoch of the second variable term. Thus putting $H_1=13^h\ 44^m$ we find

$a=3{\cdot}369$, $A=240°\ 42'$, $b=0{\cdot}61$, $B=53°\ 57'$ and the minimum at $3^h\ 36^m$;

while for $H_1=13^h\ 36^m$ we find

$a=3{\cdot}368$, $A=240°\ 56'$, $b=0{\cdot}63$, $B=54°\ 46'$ and the minimum at $3^h\ 56^m$.

Plotting these curves we select the latter as possessing the more probable form of the two; thus giving, together with an absolute representation of the three fundamental observations, a curve of which the general form is normal and the epoch of minimum not improbable. These two conditions cease to exist when the assumed values of M and H_1 are much changed, and they fulfill approximately the function of that additional observation which would permit the constants to be rigorously determined.

Referring now to the series of hourly observations, we find these to be as given below. At their side are the temperatures which correspond to our approximate formula; and a glance at the column $0-C$ suffices to show how largely the residuals are due to the neglected term depending upon $3h$.

Hour.	Temperature.		$0-C$.	Hour.	Temperature.		$0-C$.
	Observed.	Computed.			Observed.	Computed.	
1^h	10°·79	10°·41	+0°·38	13^h	17°·20	17°·25	−0°·05
2	10·49	10·23	+0·26	14	17·29	17·29	0·00
3	10·29	10·18	+0·11	15	16·98	16·96	+0·02
4	10·18	10·27	−0·09	16	16·58	16·33	+0·25
5	10·05	10·68	−0·63	17	15·95	15·50	+0·45
6	10·76	11·12	−0·36	18	14·76	14·56	+0·20
7	11·90	11·90	0·00	19	13·51	13·64	−0·13
8	13·11	12·89	+0·22	20	12·56	12·79	−0·23
9	14·26	13·99	+0·27	21	12·07	12·07	0·00
10	15·30	15·10	+0·20	22	11·67	11·50	+0·17
11	16·13	16·09	+0·04	23	11·33	11·03	+0·30
11	16·89	16·84	+0·05	24	11·04	10·68	+0·36

The true formula as deduced from the hourly observations is

$$T=13{\cdot}38+3{\cdot}536\ \sin\ (h+239°\ 33')+0{\cdot}645\ \sin\ (2h+66°\ 1')+0{\cdot}365\ \sin\ (3h+34°\ 31')$$

the corresponding maximum occurring at $13^h\ 31^m$, and the minimum at $3^h\ 26^m$. The mean error of the approximate formula is +0°·25.

3. As a third and last example we will take the important series of observations made at Upsala in Sweden during the ten years 1869–1879, and consider the mean diurnal variation during the entire year.

Here the three observations are $T_7=3\cdot25$, $T_{14}=7\cdot48$, $T_{21}=4\cdot07$, in degrees of the centigrade scale. Using for M the mean of these three temperatures we have a secondary maximum in the curve; and decreasing the assumed daily mean until the tendency to abnormal flexure disappears, we furthermore perceive that the epoch of maximum is probably later than 14^h.

Supposing then this epoch to be $14^h\ 36^m$, we find for $M=4\cdot70$, $a=2.29$, $A=228°$, $b-0\cdot52$, $B=20°$, with an evident tendency to a secondary maximum and a very flattened curve for the night-hours. Even for $M=4\cdot65$ the same characteristics are recognizable. But for $M=4\cdot60$, for which the curve appears satisfactory in all other respects, we find the improbable epoch $2^h\ 42^m$ for the minimum.

It is furthermore readily seen that either an increased value of M, or an earlier value of H, will give a later epoch of minimum. Thus we have for

M=4·65	$H_1=14^h$	36^m	$a=2\cdot45$	A=228°	55′	$b=0\cdot43$	B=18°	17′	$H_2=3^h$	24^m
4·60	14	36	2·60	229	43	0·33	17	1	2	42
4·60	14	24	2·60	230	41	0·32	31	28	3	15
4·60	14	12	2·58	231	34	0·33	45	19	3	24
4·58	14	24	2·66	230	56	0·28	32	40	3	20

Plotting these curves, it is clear that either of the last three will give satisfactory results, although the general form of the last but one seems the more probable.

Comparing the observed values with those deduced from this system of constants, we find

Hour.	Temperature.		O—C.	Hour.	Temperature.		O—C.
	Observed.	Computed.			Observed.	Computed.	
1^h	2°·55	2°·55	0°·00	13^h	7°·28	7°·29	−0°·01
2	2·30	2·36	−0·06	14	7·48	7·48	0·00
3	2·10	2·27	−0·17	15	7·45	7·41	+0·04
4	2·02	2·28	−0·26	16	7·20	7·08	+0·12
5	2·19	2·43	−0·24	17	6·78	6·59	+0·19
6	2·60	2·75	−0·15	18	6·15	5·97	+0·18
7	3·25	3·25	0·00	19	5·46	5·31	+0·15
8	4·01	3·90	+0·11	20	4·75	4·66	+0·09
9	4·84	4·65	+0·19	21	4·07	4·07	0·00
10	5·64	5·47	+0·17	22	3·54	3·57	−0·03
11	6·36	6·23	+0·13	23	3·13	3·15	−0·02
12	6·91	6·86	+0·05	24	2·82	2·82	0·00

The true value of M, as determined by the hourly observations, is 4·62, the maximum being at $14^h\ 10^m$ and the minimum

at $3^h\ 22^m$. The mean discordance $0°-C.$ deduced from the table of comparison is $\pm 0°{\cdot}13$ which would be reduced to $\pm 0°{\cdot}10$ by adding a third term, $0°{\cdot}10 \sin (3^h+45°)$.

These examples will show how by a rough use of tentative processes a tolerable approximation to the true form of the diurnal curve may be obtained from observations made three times daily. The labor, though often considerable, is by no means so great in actual practice as might be inferred from the detailed description. A very little experience and tact are sufficient to indicate to the computer those hypothetical values which will guide him most readily to the desired end, this being the recognition of those limits for the unknown quantities within which they are not contraindicated and the selection of a probable curve within these, generally narrow, limits.

I do not fear that any unbiased person will misapprehend the claims here made in behalf of this mode of inquiry. It is of course only approximate; but in regions where the climatic relations have previously been unknown, it is certainly of high importance to obtain, from such data as may be accessible, determinations which are demonstrably not far from the truth; even though they may be destitute of that exactness which can only be obtained from the mean of many daily observations through an extended series of years.

But so soon as we are in possession of knowledge of other fundamental data to be combined with the three daily observations,—when for example we may infer from analogy the probable form of the true daily curve, or know the epoch or the temperature for the maximum or minimum, within moderate limits,—we may not only infer with confidence the constants of the second term, but may often deduce very fair values for the third.

Under ordinary circumstances this is not difficult, and it may therefore be well to consider the present state of our knowledge regarding the epochs of daily maximum and minimum. We will endeavor to avoid misplaced refinements of calculation, since attempts at minuteness in determining these epochs are for the most part justified neither by the nature of the phenomenon nor by the character of the observations; but will take into consideration only the unmistakable fact of the case.

The fact that the epochs of the daily extremes are to a high degree vague and uncertain cannot escape the most inattentive observer. It is of course the duty of the meteorologist to fix their typical values and the limits of their normal variation with such degree of precision as the nature of the case permits; still all endeavors must prove vain which aim at determining them with the minuteness which belongs to the epoch either of a sharply defined or of a tolerably regular phenome-

non. Cases are by no means infrequent in which the highest temperature of the day occurs as early as 8 A. M., or as late as 8 P. M. Sudden changes of wind, or in the amount of cloudiness exert so great an influence that great care is often needful for obtaining values for the epochs which shall be even approximately near to the normal ones. And allusion has already been made to the very great length of the period which would be requisite for determining these values with any precision. Two successive periods of ten years each might easily give average times for the daily maximum, differing by many minutes, whether the interval considered be one of 5, 7, 10, or 30 days, or the entire year. The next following would, in its turn, generally differ from both the previous ones; indeed the duration of the observations needed for obtaining a value, which should be essentially unchangeable by their longer continuance, is such that it has certainly been obtained for few, if indeed for any, places on the earth's surface. This certainly does not absolve us from the duty of investigating and remedying the sources of constant errors which would not probably disappear from the mean of a larger number of observations; but none the less is it futile to attempt to fix the epochs within limits which nature has not prescribed. To these considerations is to be added the independent one that, in many places, the diurnal curve of temperature varies so slightly at certain seasons in the vicinity of its extremes that a point of maximum or minimum value has scarcely more than a theoretical existence; so that small, and often inevitable, errors of observation would suffice to change the epochs by an amount relatively very large.

If determinations of the average times of the epochs are to be made within narrow limits of error and in such a way as to be serviceable for scientific ends, they must either be separately investigated for clear weather and cloudy, high barometer and low, north wind and south, dry air and moist, hot days and cool ones,—or else, what is far less practicable, observations must be employed which extend over a period sufficiently long for the effects of all such varying conditions to be eliminated from their mean results. Moreover, exact values, even though attainable, would contribute comparatively little to the progress of discovery, unless accompanied by determinations of the extent of their normal variation, and of the amount to which they are modified by influences analogous to those just mentioned.

The distinguished physicist, to whose criticisms and strong denunciations of the employment of the general mathematical formula we have alluded, has repeatedly asserted that errors in the times of daily maximum and minimum are produced by the employment of Bessel's formula; and that this places the

minima too early. Indeed he attempts to demonstrate these supposed facts by numerical illustrations derived from observations made at Katharinenburg in April for 18 years, and at Tiflis in May for 10 years. The deserved influence of Dr. Wild in all that relates to meteorological investigation gives to his opinion and counsel an importance which calls for a careful disproof of his mistakes in this respect, lest the progress of research be seriously impeded by the proposed disregard of algebraic generalization.

That a general mathematical formula, which absolutely represents all the fundamental observations, must afford more correct results than can be obtained from the same observations in any other way, would seem too nearly a truism to require mention. If graphical determinations, based on the same data as the numerical ones, differ from these, they must be erroneous; yet it is asserted (Temp. Verhältn, p. 61) that even with 12 terms of the formula (24 constants) the epoch of minimum for Tiflis as deduced from hourly mean temperatures, is not correctly given. It may be admitted that Dr. Wild's tabular view might at the first glance suggest such interpretation; and, furthermore, conceded that the small numerical errors,* which have apparently given him the impression that the formula failed to represent the observations, do not essentially affect the gradual advance of the epoch of minimum, as the several terms of the formula are successively incorporated in the computation. In this particular case it would appear that nearly the whole twelve terms are requisite for giving the correct epoch of mean minimum; if so, we must of course accept the fact, and investigate the influences to which it is due. These we cannot but believe to be exceptional, and to arise either from local peculiarities or errors of observation, — aggravated perhaps by the employment of an inadequate number of years.

Well founded exceptions might be taken to the way in which these results have been obtained, since they are not deduced from the pure observations, but from data which have been modified by an empirical correction, for the purpose of eliminating the so-called influence of annual variation. The course pursued has been practically equivalent to dividing the difference of the normal mean temperature for the first and last days of the month by the number of intermediate hours; and then by means of the hourly difference, thus obtained, modifying twenty-three out of the twenty-four daily observations in order to refer them to the epoch of the remaining one by applying the supposed amount of annual variation during the interval. The various statistical tabulations of the daily, monthly,

* These, although insignificant in themselves, are in fact sufficient to change the corresponding epoch of maximum by thirteen minutes, or from $14^h\ 15^m$ to $14^h\ 28^m$.

and yearly results are thus made numerically accordant; but at a sacrifice of real accuracy. The corrections thus applied have the effect of belatening the minimum in the spring half of the year and anticipating it in autumn. For it cannot seriously be maintained that the influence of an increase or decrease of the sun's declination continues through the night-hours as through the day. This influence is necessarily intermittent; in dealing with annual variations, we cannot use hours as the independent variables; although here also the numerical changes are small, and although their influence disappears from the daily means and from determinations based upon them, yet whatever effect they may have goes to produce distortion and error in the resultant curve of diurnal variation. In this very case of May at Tiflis, the observations themselves give $4^h\ 47^m{\cdot}3$ for the mean epoch of minimum, instead of $4^h\ 50^m{\cdot}5$, which results after they have been modified by this reduction for annual variation, or of $5^h\ 0^m$ as deduced graphically by Dr. Wild. This modification has a still greater effect upon the time of mean maximum, for it changes this epoch from its true value $14^h\ 18^m{\cdot}8$ to $14^h\ 28^m$ or to $14^h\ 32^m$ if we accept the graphical result.

But, leaving this relatively unimportant question, the most striking fact to be noted in the example now considered is that the form of the diurnal curve, in the vicinity of its maximum, is such that, while the minimum temperature—$4°{\cdot}60$, as given by only three variable terms, corresponds to an epoch $4^h\ 17^m{\cdot}6$, yet the analogous value—$4°{\cdot}76$, deduced from the full series of twelve terms, and only $0°{\cdot}16$ lower, corresponds to the epoch $4^h\ 50^m{\cdot}5$, more than half an hour later. That obtained graphically and given as the correct one by Dr. Wild, is, as above mentioned, $5^h\ 0^m$.

The accomplished physicist, whose views we reluctantly oppose, maintains that his method gives the epoch with accuracy to within 2^m. With reference to this we will only remark that careful trials, plotting the observations with the utmost care upon the scale employed by Dr. Wild, show that the graphical method could give any value from $4^h\ 48^m$ to $5^h\ 12^m$ according to the taste or fancy of the draughtsman. Tracing the curve as it results from the formula, its true form may be recognized. If graphical processes are to be trusted in such cases, they must be based upon more frequent observations:—half-hourly, at least, in case the result is to represent the actual mean epoch within eight or ten minutes. How inordinately the resultant epoch would be affected by the, certainly not improbable, error of a few hundredths of a degree in the mean observed temperature, either for 4^h or for 5^h, is manifest. It may not be amiss to add that the mean discordance between the observed and

calculated temperatures, using only three variable terms of the formula, is less than the tenth of a degree. Using five variable terms, it is less than four hundredths of a degree.

In case the preceding month, April, had been selected for the illustration instead of this exceptional one of May, we should have found the time of minimum when obtained by the use of only six variable terms to be one minute *later* than if deduced from the complete series.

Or if the next following month, June, had been taken, the formula with only five variable terms would have given the epoch of minimum not only within a single minute of that resulting from the full series, but also *later* than any of those resulting from the successive incorporation of the four following terms.

When we examine the results obtained for Katharinenburg, the case is analogous, but even more favorable for illustrating the facts to which we call attention. Here the formula with but five variable terms represents the observations with such completeness that the mean discordance amounts to only 0°·02. With eight variable terms the discordance does not exceed one one-hundredth of a degree at any hour from 2 to 7 A. M.; and the same minute results for the epoch of minimum as when the complete formula is used. Indeed, with only six variable terms, the difference does not exceed five minutes. The value obtained graphically by Dr. Wild is five minutes later than that which results from the formula using the same data; and careful trials, employing the scale used by him, have convinced me that curves may be so drawn as to seem equally plausible, while their times of minima vary by some fifteen minutes. The experiment can easily be tried by any one for himself; and it will be found that the errors, inevitably committed in the plotting and reading off, are far less than those resulting from the sketching of the curve.

The extreme variability and uncertainty of these mean epochs of diurnal maxima and minima find an excellent illustration in the values corresponding to successive years in the admirable series of hourly observations made at Upsala under the direction of Dr. Hildebrandson. During the eight years 1869–1876, the mean epoch of daily minimum, as deduced from all the observations of the year, fluctuated between the limits $3^h\ 32^m$, and $4^h\ 12^m$, and that of maximum from $13^h\ 51^m$, to $14^h\ 42^m$. Dr. Wild himself acknowledges that the mean epoch of daily minimum as deduced from a whole year's observations was "tolerably constant," when it varied by 50^m during the years from 1857 to 1862. [Temp. Verh., p. 54].

It has already been stated that in the trials for obtaining the daily mean, the time of maximum may, in the absence of any

knowledge, be at first advantageously assumed as 2 P. M.; and also show how a rough approximate knowledge of the epochs may be made to contribute essentially to that of the formula.

To aid the attainment of this let us consider the facts at our disposal regarding the values of these epochs in general.

Very extensive materials are available for this purpose, but it is safer to restrict ourselves to a limited number of points for which the observations have been scrutinized, and the inferences deduced, with special care, than to base our researches upon a larger number of less trustworthy results. For these reasons the values adopted by Dr. Wild in the work already cited, have generally been preferred to others; and those for a number of places, such as Upsala, Berne, Leipzig, Greenwich, St. Helena, Lisbon, have been specially determined here. All depend upon hourly observations, with the single exception of Schwerin; but at Santiago de Chile, and Valparaiso, the observations were made during only a few days in each month, in different years. At Santiago the total number of days' of observation was 130, at Valparaiso, 60. To facilitate the examination of the results here given, and the deduction of analogous ones for other places, it may be convenient for those who are not astronomers, to have ready access to general tables of the equation of time, the sun's declination, and the times of sunrise and sunset in different latitudes. Those which follow* represent the mean values; and although not strictly accurate in every year, they may be used without hesitation for meteorological purposes. The equation of time as here given is to be applied to the mean time, to obtain the apparent; or with reversed sign to the apparent if the mean is desired.

The first of these tables gives the mean values for each decade during the year, corresponding to our arrangement, according to which all the days subsequent to the 20th of each month are made to form the third decade. The second table gives the apparent time of sunrise if the latitude be North, and of sunset if it be South, for the middle of each month.

Our data relative to the mean epochs of the diurnal extremes are deduced from the observations in each month separately during the period considered, and also from the annual means; the number of years of observation employed being mentioned in each case. The values are given in apparent time; those of the observed maxima being counted from apparent noon, and those of the minima being given by the number of minutes before sunrise, for which moment the arrival of the sun's center at the horizon is used, without any correction for refraction.

* The tables mentioned are given in the original article, but are necessarily omitted here.—EDS.

The data thus collected afford some opportunity for generalization; but this we reserve for some future occasion; simply calling attention to the relatively small extent of deviation of the average epoch of maximum from 2 P. M., and to the large amount by which the average times of minimum, as deduced from the whole number of observations, differ from the mean of those deduced for the several months. The general tendency of the interval, between the minima and true sunrise, to increase with the latitude is also readily perceptible. This is of course due to the sun's near approach to the horizon long before its actual arrival there.

Desiring a rough test both of the average epoch of maximum, as deduced from a short period of observation, and of its ordinary extent of variation, I wrote in April, 1880, to several of the gentlemen, at different places, to whom we are indebted for our meteorological observations, asking them to note the temperature half-hourly for a period of some hours which should include the epochs of maximum during ten or twelve successive days.

These observations referred to apparent time give the following results:

Place.	Approx. Lat.	Period.	Mean Epoch of Maximum.	Uncertainty of Determinat'n.	Variation.
Bahia Blanca	38° 45′	May 12–23	$2^h\ 13^m$ P. M.	$5\frac{1}{2}^m$	$1^h 27^m - 2^h 48^m$
Buenos Aires	34 16	Apr. 21–30	2 22	$4\frac{1}{2}$	1 2 –2 56
S. Antonio de Areco	34 13	Apr. 21–30	2 $19\frac{1}{2}$	3	1 38 –2 44
Cordoba	31 25	May 1–10	2 14	$3\frac{1}{2}$	1 27 –3 51
		11–20	2 11	6	11 28 –3 40
Villa Hernandarias	31 15	May 1–14	2 $45\frac{1}{2}$	3	1 40 –3 28
Goya	29 9	May 1–10	1 $55\frac{1}{2}$	5	12 48 –2 48
Salta	24 50	Apr. 23–30	2 38	4	1 14 –2 57

The column entitled "Uncertainty of Determination," contains the sum of the various estimated possible errors in plotting the observations, drawing the curves, and reading of the results. The limits of probable, or possible, errors of the last two classes depend of course, principally, upon the form of the curve itself. The column entitled "Variation," contains the difference between the extreme values on different days during the continuance of the series.

It will be seen that these results confirm our previous inferences.

That the later terms of the formula should become more important in high latitudes during the winter season is a manifest consequence of the inequality of the two portions into which the minimum divides the interval between the normal epochs of maximum. When sunrise occurs so early as to render this

inequality comparatively slight, the first variable term, the period of which is twelve hours, acquires an overwhelming significance; and the residuals which it leaves are disposed of with comparative ease by the other terms of long period. But when the sun rises later the reverse must be the case. By means of the data above collected, provisional assumptions of the form of the diurnal curve may often be so made as to afford important assistance in deducing its true form, with great approximation, from a small number of observations.

On the other hand, no argument is needed to show how unjustifiable is that frequent misuse of the general formula by which the values deduced for certain terms from observations made during the day time or at inadequate intervals, are employed to represent the nocturnal portion of the curve. Indeed it is usually the most important terms which are thus least accurately determined. Bi-hourly observations will give the constants as far as the 6th term; hourly ones, will give them as far as the 12th; but these same arguments, which call for hourly observations for accurate determination of the epochs, apply with equal force to semi-hourly ones, for a more exact determination still. How far the game is worth the candle, is an entirely distinct question.

Summing up the several conclusions at which we have arrived we find as follows:

1. The formula, depending upon the sines and cosines of multiples of the time since an arbitrary epoch, is a very simple, —probably the simplest,—form of expressing the law, of a cyclical variation of this class. It is not a mere formula of interpolation; nor can it, when legitimately used, give rise to any systematic error whatsoever.

2. If in such variation there be a general law, this will be made manifest by the relative inappreciableness of the higher terms; and the chief function of these will be to equate out the errors of observation.

3. Although the results of hourly observations may be absolutely expressed by the formula with twenty-four constants, yet the values given by such a formula would probably be in general less correct than those resulting from the use of fewer terms, first, because it would imply an absence of law in the variation; secondly, because it would necessarily include all errors of observation to their full extent, instead of equating them out.

4. Three daily observations, tolerably well distributed, together with an approximate value of the epoch of maximum and the condition of only one point of contrary flexure in the curve, give a close approximation to the true formula for the daily variation, so far as this can be expressed without includ-

ing those terms which complete their period in less than eight hours.

5. An approximate knowledge of the epoch of the maximum or minimum temperature, will, for each of them, enable us to add an additional constant to the formula with a good degree of approach to the truth.

6. The mean epochs of maximum or minimum can seldom be determined with precision even by employing the highest refinements of observation and calculation known to science. Indeed it appears unlikely that they ever have been ascertained for any points of the earth's surface with less uncertainty than several minutes. It is moreover questionable whether there are any such epochs sufficiently marked to permit determinations without the introduction of various conditions; unless by the employment of observations extending over a series of years sufficiently long to eliminate all the variety of conditions. Even if this be possible it will not be within the attainment of the present generation or their near posterity.

ART. XI.—*Celestial Chemistry from the Time of Newton;* by T. STERRY HUNT, LL.D., F.R.S.*

THE late W. Vernon Harcourt, in 1845,† called attention to the remarkable perception of great chemical truths which is apparent in the Queries appended to the third book of Newton's *Optics*, as well as in his Hypothesis touching Light and Color. With regard to the latter, Harcourt then remarked, "it has, I think, scarcely been quoted, except by Dr. Young, and its existence is but little known, even among the best-informed scientific men." The essay in question was read before the Royal Society, December 9th and 16th, 1675, but remained unpublished till 1757, when Birch, at that time secretary to the Society, printed it, not without verbal inaccuracies, in the third volume of his History of the Royal Society; a work intended to serve as supplement to the Philosophical Transactions up to that date. In 1846, at the suggestion of Harcourt, the Hypothesis of Newton was again printed in the L. E. and D. Philosophical Magazine (volume xxix), and it subsequently appeared in the Appendix to the first volume of Brewster's Memoirs of Sir Isaac Newton, in 1855.

The time has come for further inquiries into the science of Newton, and I shall endeavor to show that a careful examina-

* Read before the Cambridge (England) Philosophical Society, November 28, 1881, and reprinted from its Proceedings.

† L. E. and D. Philos. Magazine, III, xxviii, 106 and 478; also xxix, 185.

tion of the writings of our great Natural Philosopher, in the light of the scientific progress of the last generation, renders still more evident the wonderful prevision of him who already, two centuries since, had anticipated most of the recent speculations and conclusions regarding cosmic chemistry.

As an introduction to the inquiries before us, and in order to show the real significance of the speculations of Newton, it will be necessary to review, somewhat at length, the history of certain views enunciated almost simultaneously by the late Sir Benjamin Brodie, of Oxford, and the present writer, and subsequently developed and extended by the latter. In part I of his Calculus of Chemical Operations, read before the Royal Society, May 3, 1866, and published in the Philosophical Transactions for that year, Brodie was led to assume the existence of certain ideal elements. These, he said "though now revealed to us through the numerical properties of chemical equations only as *implicit and dependent existences*, we cannot but surmise may sometimes become, or may in the past have been, *isolated and independent existences*." Shortly after this publication, in the spring of 1867, I spent several days in Paris with the late Henri Sainte-Claire Deville, repeating with him some of his remarkable experiments in chemical dissociation, the theory of which we then discussed in its relations to Faye's solar hypothesis. From Paris, in the month of May, I went, as the guest of Brodie, for a few days to Oxford, where I read for the first time and discussed with him his essay on the Calculus of Chemical Operations, in which connection occurred the very natural suggestion that his ideal elements might perhaps be liberated in solar fires, and thus be made evident to the spectroscope. I was then about to give, by invitation, a lecture before the Royal Institution on The Chemistry of the Primeval Earth, which was delivered May 31, 1867. A stenographic report of the lecture, revised by the author, was published in the Chemical News of June 21, 1867, and in the Proceedings of the Royal Institution. Therein, I considered the chemistry of nebulæ, sun and stars in the combined light of spectroscopic analysis and Deville's researches on dissociation, and concluded with the generalization that the "breaking-up of compounds, or dissociation of elements, by intense heat is a principle of universal application, so that we may suppose that all the elements which make up the sun, or our planet, would, when so intensely heated as to be in the gaseous condition which all matter is capable of assuming, remain uncombined; that is to say, would exist together in the state of chemical elements; whose further dissociation in stellar or nebulous masses may even give us evidence of matter still more elemental than that revealed in the experiments of the laboratory, where we can

only conjecture the compound nature of many of the so-called elementary substances."

The importance of this conception, in view of subsequent discoveries in spectroscopy and in stellar chemistry, has been well set forth by Lockyer in his late lectures on Solar Physics,* where, however, the generalization is described as having been first made by Brodie in 1867. A similar but later enunciation of the same idea by Clerk-Maxwell is also cited by Lockyer. Brodie, in fact, on the 6th of June, one week after my own lecture, gave a lecture on Ideal Chemistry before the Chemical Society of London, published in the Chemical News of June 14th, in which, with regard to his ideal elements, in further extension of the suggestion already put forth by him in the extract above given from his paper of May 6, 1866, he says "we may conceive that in remote ages the temperature of matter was much higher than it is now, and that these other things [the ideal elements] existed in the state of perfect gases—separate existences—uncombined." He further suggested, from spectroscopic evidence, that it is probable that "we may one day, from this source have revealed to us independent evidence of the existence of these ideal elements in the sun and stars."

During the months of June and July, 1867, I was absent on the continent, and this lecture of Brodie's remained wholly unknown to me until its republication in 1880, in a separate form, by its author,† with a preface, in which he pointed out that he had therein suggested the probable liberation of his ideal elements in the sun, referring at the same time to his paper of 1866, from which we have already quoted the only expression bearing on the possible independence of these ideal elements somewhere in time or in space.

The above statements are necessary in order to explain why it is that I have made no reference to Sir Benjamin Brodie on the several occasions on which, in the interval between 1867 and the present time, I have reiterated and enforced my views on the great significance of the hypothesis of celestial dissociation as giving rise to forms of matter more elemental than any known to us in terrestrial chemistry. The conception, as at first enunciated in somewhat different forms alike by Brodie and myself, was one to which we were both naturally, one might say inevitably, led by different paths from our respective fields of speculation, and which each might accept as in the highest degree probable, and make, as it were, his own. I write, therefore, in no spirit of invidious rivalry with my honored and lamented friend, but simply to clear myself from the charge, which might otherwise be brought against me, of

* Nature, August 25, 1881, vol. xxiv, p. 396.

† Ideal Chemistry, a Lecture. Macmillan, 1880.

having on various occasions within the past fourteen years, put forth and enlarged upon this conception without mentioning Sir Benjamin Brodie, whose only publication on the subject, so far as I am aware, was his lecture of 1867, unknown to me until its reprint in 1880.

It was at the grave of Priestley, in 1874, that I for the second time considered the doctrine of celestial dissociation, commencing with an account of the hypothesis put forward by F. W. Clarke, of Cincinnati, in January, 1873,* to explain the growing complexity which is observed when we compare the spectra of the white, yellow and red stars; in which he saw evidence of a progressive evolution of chemical species, by a stoichiogenic process, from more elemental forms of matter. I then referred to the further development of this view by Lockyer in his communication to the French Academy of Sciences in November of the same year, wherein he connected the successive appearance in celestial bodies of chemical species of higher and higher vapor-densities with the speculations of Dumas and Pettenkofer as to the composite nature of the chemical elements.† I then quoted from my lecture of 1867 the language already cited, to the effect that dissociation by intense heat in stellar worlds might give us more elemental forms of matter than any known on earth, and further suggested that the green line in the spectrum of the solar corona, which had been supposed to indicate a hitherto unknown substance, may be due to a "more elemental form of matter, which, though not seen in the nebulæ, is liberated by the intense heat of the solar sphere, and may possibly correspond to the primary matter conjectured by Dumas, having an equivalent weight one-fourth that of hydrogen." The suggestion of Lavoisier, that "hydrogen, nitrogen, and oxygen, with heat and light, might be regarded as simpler forms of matter from which all others are derived," was also noticed in connection with the fact that the nebulæ, which we conceive to be condensing into suns and planets, have hitherto shown evidences only of the presence of the first two of these elements, which, as is well-known, make up a large part of the gaseous envelope of our planet, in the forms of air and aqueous vapor. With this, I connected the hypothesis that our atmosphere and ocean are but portions of the universal medium which, in an attenuated form, fills the interstellary spaces; and further suggested as "a legitimate and plausible speculation," that "these same nebulæ and their resulting worlds may be evolved by a process of chemical condensation from this universal atmosphere, to which they would sustain a relation some-

* Clarke, "Evolution and the Spectroscope," Popular Science Monthly, New York, vol. ii, p. 32.

† Lockyer, Comptes Rendus, November 3, 1873.

what analogous to that of clouds and rain to the aqueous vapor around us."*

These views were reiterated in the preface to a second edition of my Chemical and Geological Essays, in 1878, and again before the British Association for the Advancement of Science at Dublin,† and before the French Academy of Sciences in the same year.‡ They were still further developed in an essay on the Chemical and Geological Relations of the Atmosphere, published in this Journal for May, 1880, in which attention was called to the important contribution to the subject by Mr. Lockyer in his ingenious and beautiful spectroscopic studies, the results of which are embodied in his "Discussion of the Working Hypothesis that the so-called Elements are Compound Bodies," communicated to the Royal Society, December 12, 1878. It was then remarked that the already noticed "speculation of Lavoisier is really an anticipation of that view to which spectroscopic study has led the chemists of to-day;" while it was said that the hypothesis put forth by the writer in 1874, "which seeks for a source of the nebulous matter itself, is perhaps a legitimate extension of the nebular hypothesis."

To show the connection of the above views with the philosophy of Newton, it now becomes necessary to give some account of the conception of the universal distribution of matter throughout space, both as regards its dynamical relations and its chemical composition. Passing over the speculations of the Greek physiologists, we come to the controversies on this subject in the seventeenth century, and find, in apparent opposition to the plenum maintained by Descartes and his followers, the teaching of Newton that "the heavens are void of all sensible matter." This statement is, however, qualified elsewhere by his assertion, that "to make way for the regular and lasting movements of the planets and comets, it is necessary to empty the heavens of all matter, except perhaps some very thin vapors, steams and effluvia arising from the atmospheres of the earth, planets and comets, and from such an exceedingly rare etherial medium as we have elsewhere described," etc. (*Optics*, Book III, Query 28).

In order to understand fully the views of Newton on this subject, it is necessary to compare carefully his various utterances, including the Hypothesis, in 1675, the first edition of the *Principia*, in 1687, the second edition, in 1713, and the various editions of the *Optics*. This work appeared in 1704, the third book, with its appended queries, having, according to its author's preface, been "put together out of scattered papers"

* A Century's Progress in Theoretical Chemistry, being an address at Northumberland, Penn., July 31, 1874; Amer. Chemist, vol. v, pp. 46–61, and Pop. Science Monthly, vi, p. 420.

† Nature, Aug. 29, 1878, vol. xviii, p. 475.

‡ Comptes Rendus, Sept. 23, 1878, vol. xxxviii, p. 452.

subsequent to the publication of the first edition of the *Principia.* The Latin translation of the *Optics*, by Dr. Clarke, which was published in 1706, and the second English edition, in 1718, contain successive additions to these queries, which are indicated in the notes to Horsley's edition of the works of Newton, and are important in this connection. From a collation of all these, we learn how the conceptions of the Hypothesis took shape, were re-inforced, and in great part incorporated in the *Principia.*

In the Hypothesis, he imagines "an etherial medium much of the same constitution with air, but far rarer, subtler, and more elastic." "But it is not to be supposed that this medium is one uniform matter, but composed partly of the main phlegmatic body of ether, partly of other various etherial spirits, much after the manner that air is compounded of the phlegmatic body of air intermixed with various vapors and exhalations." Newton further suggests in his Hypothesis that this complex spirit or ether, which, by its elasticity, is extended throughout all space, is in continual movement and interchange. "For nature is a perpetual circulatory worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fixed, subtile out of gross, and gross out of subtile; some things to ascend and make the upper terrestrial juices, rivers, and the atmosphere, and by consequence others to descend for a requital to the former. And as the earth, so perhaps may the sun imbibe this spirit copiously, to conserve his shining, and keep the planets from receding farther from him; and they that will may also suppose that this spirit affords or carries with it thither the solary fuel and material principle of life, and that the vast etherial spaces between us and the stars are for a sufficient repository for this food of the sun and planets."

The language of this last sentence, in which his late biographer, Sir David Brewster, regards Newton as "amusing himself with the extravagance of his speculations," at which "we may be allowed to smile,"* was not apparently regarded as unreasonable by its author when, more than ten years later, he quoted it in the postscript of his letter to Halley, dated Cambridge, June 20, 1686. The views therein contained, with the single exception of the suggestion regarding gravitation, have not wanted advocates in our own time, and many of them were embodied in the *Principia*, which Newton was then engaged in writing.

But this was not all: Newton saw in the cosmic circulation and the mutual convertibility of rare and dense forms of matter a universal law, and rising to a still bolder conception, which

* Brewster's Memoirs of Newton, vol. i, pp. 121 and 404.

completes his Hypothesis of the Universe, adds: "Perhaps the whole frame of nature may be nothing but various contextures of some certain etherial spirits or vapors, condensed, as it were, by precipitation, much after the same manner that vapors are condensed into water, or exhalations into grosser substances, though not so easily condensible; and after condensation wrought into various forms, at first by the immediate hand of the Creator, and ever since, by the power of nature, which, by virtue of the command 'increase and multiply,' became a complete imitator of the copy set her by the great Protoplast. Thus, perhaps may all things be originated from ether."

If now we look to the third book of the *Principia*, we shall find in proposition 41 the remarkable chemical argument by which Newton was led to regard the interstellary ether as affording "the material principle of life" and "the food of planets." Considering the exhalations from the tails of comets, he supposes that the vapors thus derived, being rarified, dilated, and spread through the whole heavens, are by gravity brought within the atmospheres of the planets, where they serve for the support of vegetable life. Inasmuch, moreover, as all vegetation is supported by fluids, and subsequently, by decay is, in part, changed into solids, by which the mass of the earth is augmented, he concludes that if these essential matters were not supplied from some external source, they must continually decrease, and at last fail. This vital and subtile part of our atmosphere, so important, though small in amount, he then supposed might come from the tails of comets.*

This appeared in the first edition of the *Principia*, in 1687. It was not until later that the conception of exhalations from other celestial bodies took shape in the mind of Newton, as we may learn from the *Optics*. Thus, in the first edition of this work, in Query 11, the sun and fixed stars are spoken of as

*"Vapor enim in spatiis illis liberrimis perpetuò rarescit, ac dilatatur. Quâ ratione fit ut cauda omnis ad extremitatem superiorem latior sit quám juxta capita cometae. Eâ autem rarefactione vaporem perpetuò dilatatum diffundi tandem et spargi par coelos universos, deinde paulatim in planetas per gravitatem suam attrahi et cum eorum atmosphaeris misceri, rationi consentaneum videtur. Nam quemadmodum maria ad constitutionem Terrae hujus omnino requiruntur, idque ut ex iis per calorem Solis vapores copiosè satis excitentur, qui vel in nubes coacti decidant in pluviis, et Terram omnem ad procreationem vegetabilium irrigent et nutriant; vel in frigidis montium verticibus condensati (ut aliqui cum ratione philosophantur) decurrant in fontes et flumina: sic ad conservationem marium et humorum in planetis requiri videntur cometae, ex quorum exhalationibus et vaporibus condensatis, quicquid liquoris per vegetationem et putrefactionem consumitur et in Terram aridam convertitur, continuò suppleri et refici possit. Nam vegetabilia omnia ex liquoribus omninò crescunt, dein magnâ ex parte in Terram aridam per putrefactionem abeunt, et limus ex liquoribus putrefactis perpetuò decidit. Hinc moles Terrae aridae indies augetur, et liquores, nisi aliunde augmentum sumerent, perpetuò decresere deberent, ac tandem deficere. Porro suspicor spiritum illum, qui aëris nostri pars minima est, sed subtillissima et optima, et ad rerum omnium vitam requiritur, ex cometis praecipue venire."—*Newton, Principia*, lib. III, prop. XLI.

great earths, intensely heated, and surrounded with dense atmospheres which, by their weight, condense the exhalations arising from these hot bodies. To this Query is added, in 1706, the suggestion that the weight of such an atmosphere "may hinder the globe of the sun from being diminished except by the emission of light;" while in the second English edition, in 1718, we find a further addition, in the words "and a very small quantity of vapors and exhalations." A similar change of view appears in the Query now numbered 28, wherein we read of "places [almost] destitute of matter," and also that "the sun and planets gravitate towards each other without [dense] matter between." In these quotations, the two words in brackets are wanting in the edition of 1706, and first appear in that of 1718; while the language which we have in a previous page quoted from this same Query is found in the edition of 1706.

The Queries now numbered 17–24, appeared for the first time in the edition of 1718, and herein we find, in 18, the etherial medium spoken of as being "by its elastic force expanded through all the heavens." Of this medium, "which fills all space adequately," he asks, "may not its resistance be so small as to be inconsiderable," and scarcely to make any sensible alteration in the movements of the planets?* This complex ether of the interstellary space was thus, in the opinion of Newton, made up in part of matter common to the planetary and stellar atmospheres, the origin and importance of which is concisely stated in the paragraph which appears for the first time in 1713, in the second edition of the *Principia*, in the third book, at the end of proposition 42, here much augmented. In this statement, which serves to supplement and complete that already made in 1687, in proposition 41, we read, that the vapors which arise alike from the sun, the fixed stars and the tails of comets, may by gravity fall into the atmospheres of the planets, and there be condensed, and pass into the form of salts. sulphurs, (*id est*, combustible matters,) tinctures, clay, sand, coral and other terrestrial substances.†

The conception of Newton, who, while rejecting alike the plenum of the Cartesians, with its vortices, and an absolute vacuum, imagined space to be filled with an exceedingly attenuated matter, through which a free circulation of gaseous substances might take place between distant worlds, has found

* Compare this with Prop. x, Book III of the Principia.

† "Vapores autem, qui ex Sole et stellis fixis et caudis cometarum oriuntur, incidere possunt per gravitatem suam in atmosphaeras planetarum, et ibi condensari et converti in aquam et spiritos humidos, et subinde per lentem calorem in sales, et sulphura, et tincturas, et limum, et lutem, et argillam, et arenam, et lapides, et coralla, et substantias alias terrestres paulatim migrare."—*Newton, Principia*, lib. III, prop. XLII.

favor among modern thinkers, who seem to have been ignorant of his views. Sir William Grove in 1842, suggested that the medium of light and heat may be "a universally diffused matter," and subsequently, in 1843, in the chapter on Light, in his *Essay on the Correlation of Physical Forces*, concluded with regard to the atmospheres of the sun and the planets, that there is no reason "why these atmospheres should not be, with reference to each other, in a state of equilibrium. Ether, which term we may apply to the highly attenuated matter existing in the interplanetary spaces, being an expansion of some or all of these atmospheres, or of the more volatile portions of them, would thus furnish matter for the transmission of the modes of motion which we call light, heat, etc.; and possibly minute portions of the atmospheres may, by gradual accretions and subtractions, pass from planet to planet, forming a link of material communication between the distant monads of the universe." Subsequently, in his address as President of the British Association for the Advancement of Science, in 1866, Grove further suggested that this diffused matter may become a source of solar heat, "inasmuch as the sun may condense gaseous matter as it travels in space, and so heat may be produced."

Humboldt, also, in his *Cosmos*, considers the existence of a resisting medium in space, and says "of this impeding etherial and cosmical matter," it may be supposed that it is in motion, that it gravitates, notwithstanding its great tenuity, that it is condensed in the vicinity of the great mass of the sun, and that it may include exhalations from comets; in which connection he quotes from the 42nd proposition of the third book of the *Principia*. He further speaks comprehensively of "the vaporous matter of the incommensurable regions of space, whether, scattered without definite limits, it exists as a cosmical ether, or is condensed in nebulous masses and becomes comprised among the agglomerated bodies of the universe."* Humboldt also cites in this connection a suggestion made by Arago in the *Annuaire du Bureau des Longitudes* for 1842, as to the possibility of determining, by a comparison of its refractive power with that of terrestrial gases, the density of "the extremely rare matter occupying the regions of space."†

In 1854, Sir William Thomson published his note on the Possible Density of the Luminiferous Ether,‡ wherein he remarks "that there must be a medium of material communication throughout space to the remotest visible body, is a fundamental conception of the undulatory theory of light. Whether

* Cosmos, Otté's translation, Harper's ed., vol. i, pp. 82, 86.

† Ibid., vol. iii, p. 40.

‡ Trans. Roy. Soc. Edinburgh, vol. xxi, part 1; and Phil. Mag., 1855, vol. ix, p. 36.

or no this medium is (as appears to me most probable) a continuation of our own atmosphere, its existence cannot be questioned." He then attempts to fix an inferior limit to the density of the luminiferous medium in interplanetary space by considering the mechanical value of sunlight, as deduced from the value of solar radiation and the mechanical equivalent of the thermal unit. He concludes "that the luminiferous medium is enormously denser than the continuation of the terrestrial atmosphere would be in interplantary space if rarified according to Boyle's law always, and if the earth were at rest in a state of constant temperature, with an atmosphere of the actual density at its surface." The earth itself in moving through space "cannot displace less than 250 pounds of matter."

In 1870, W. Mattieu Williams published his very ingenious work entitled The Fuel of the Sun, in which, apparently without any knowledge of what had been written before with regard to an interstellary medium, he attempts to find therein the source of solar heat—the "solary fuel" of Newton. To quote his own language, "the gaseous ocean in which we are immersed is but a portion of the infinite atmosphere that fills the whole solidity of space, that links together all the elements of the universe, and diffuses among them light and heat, and all the other physical and vital forces which heat and light are capable of generating" (loc. cit. p. 5).

Since the days of Newton, however, no one had hitherto considered the interstellary matter from a chemical point of view. In 1874, as already shown, the writer had, in extension of the conception of Humboldt that its condensation gives rise to nebulæ, ventured the suggestion that from an etherial medium having the same composition as our own atmosphere, the chemical elements of the sun and the planets have been evolved, in accordance with the views of Brodie, Clarke, and Lockyer, by a stoichiogenic process; so that in the language of Newton's Hypothesis, "all things may be originated from ether."

It was not, however, until 1878, that, from a consideration of the chemical processes which have gone on at the earth's surface within recorded geological time, I was led to another step in this inquiry. That all the de-oxidized carbon found in the earth's crust in the forms of coal and graphite, as well as that existing in a diffused state, as bituminous or carbonaceous matter, has come, through vegetation, from atmospheric carbonic acid, appears certain. To the same source we must ascribe the carbonic acid of all the limestones which, since the dawn of life on our earth, have been deposited from its waters. It is through the sub-aërial decay of crystalline silicated rocks, and the direct formation of carbonate of lime, or of carbonates of magnesia and alkalies which have reacted on the calcium-salts of the pri-

meval ocean, that all limestones and dolomites have been generated. These, apart from the coaly matter, hold, locked up and withdrawn from the aërial circulation, an amount of carbonic acid which may be probably estimated at not less than 200 atmospheres equal in weight to our own. That this amount, or even a thousandth part of it, could have existed at any one time in our terrestrial atmosphere since the beginning of life on our planet is inconceivable, and that it could be supplied from the earth's interior is an hypothesis equally untenable.

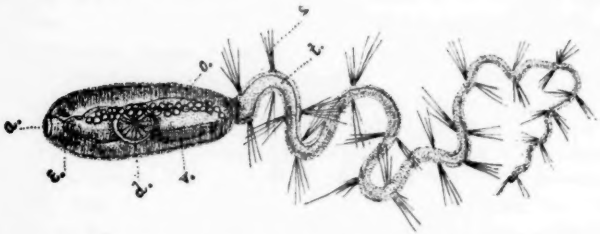
I was therefore led to admit for it an extra-terrestrial source, and to maintain that the carbonic acid has thence gradually come into our atmosphere to supply the deficiencies created by chemical processes at the earth's surface. Since similar processes are even now removing from our atmosphere this indispensable element, and fixing it in solid forms, it follows that except volcanic agency, which can only restore a portion of what was primarily derived from the atmosphere, there are on earth, besides organic decay, only the artificial processes of human industry which can furnish carbonic acid; so that but for a supply of this gas from the interstellary spaces now, as in the past, vegetation, and consequently animal life itself, would fail and perish from the earth, for want of this "food of planets."

Such were the conclusions, based on an induction from the facts of modern chemistry and geology, which I enunciated in my papers in 1878 and 1880, already quoted in the first part of this essay. I was at that time unacquainted with the Hypothesis of Newton, and with his remarkable reasoning contained in the 41st proposition of the third book of the *Principia*, in which he, so far as was possible with the chemical knowledge of his time, anticipated my own argument, and showed how and in what manner the interstellary ether may really afford the "food of planets," and, in a sense, "the material principle of life."

I have thus endeavored to bring before the Philosophical Society of Cambridge, a brief history of the development of this conception of an interstellary medium, and to show that the thought of two centuries has done little more than confirm the almost forgotten views of Newton. It is with feelings of peculiar gratification that I have been able to indite these pages within the very walls of the college in which our great philosopher lived and labored, and where, combining all the science of his time with a foresight which seems well-nigh divine, he was enabled, in the words of the poet, "to think again the great thought of the creation."

Art. XII.—*A Cercaria with Caudal Setæ;* by J. Walter Fewkes.

The interesting larva of a Trematode worm, described below, was found during my work last summer in the private laboratory of Mr. Alex. Agassiz at Newport, R. I. While watching the water in a glass jar, for the purpose of detecting new forms of pelagic life, my attention was attracted by the strange motions of a larva swimming near the surface. At first sight the unknown animal was mistaken for the nauplius of one of our common Cirripeds, which it closely resembles in the character of its movements. A microscopic examination, however, shows that it is a Cercaria, or larval Trematode, although it differs from any of these animals yet described. As nothing similar to it is to be found in the published figures of larval Trematodes, a figure and description is given below. The especially interesting feature of this Cercaria, and one which seems to me to justify this isolated publication of a description, is found in the annelid character of the tail. The larva, from this characteristic, may indicate some relationship which has not yet been pointed out, between the worms known as the Trematoda and the Annelida.



This Cercaria is marine in habitat, and is always found at or near the surface of the water. The length, when the body and tail are extended, is about one-sixteenth of an inch. Its motion through the water is never direct but, so far as was observed, consists of a "jerky" motion, brought about by the powerful strokes of its very muscular tail. This motion resembles very closely that of the nauplius of Balanus. The larva is just visible to the unaided eye and its body walls are very transparent. Under the microscope, with moderate magnifying powers, the contortions and motions of the tail are so rapid that they cannot be followed by the eye. As in all Cercariæ, the two marked divisions of the larva are the head and the tail.

The head or body is in no respect characteristic. Its shape is very variable, being sometimes contracted into a spherical

ball and at other times extended as represented in the figure. At the extremity of the head there is a mouth (*a*), which is sometimes seen to open and close. It is a circular orifice in which no proboscis could be seen. The stomach occupies a large part of the anterior central part of the body, and from it there is continued backward, a pair of blindly ending vessels, as in other Cercariæ. One of these cæca is shown in the figure (*v*). Many spherical oil globules (*o*), arranged in a row following the course of the cavity of the body, conceal the second.

A pair of black pigment patches (*e*) is found near the anterior extremity of the body. The most prominent structure of the body of the worm is a large medially placed sucker (*d*), which is situated superficially to the posterior end of the stomach near its bifurcation into the two vessels (*v*).

The tail is the most peculiar structure of this larval Trematode. Its general shape is hardly characteristic, and it owes its important interest to the appendages (*s*), found along its entire length. It is very muscular and extremely active.

The appendages (*s*) distinguish this larva from all other Cercariæ. At intervals along the whole length of the tail we find bundles of setæ arranged on opposite sides. These setæ, of which there are many in each bundle, are straight, inflexible and moved by muscles in the walls of the tail. Their resemblance to the setæ found in the segments of annelids is very great.

Ten specimens of this Cercaria were captured. It was impossible to raise them and all the specimens died soon after capture.

ART. XIII.—*Notice of the remarkable Marine Fauna occupying the outer banks off the Southern coast of New England, No.* 3; by A. E. VERRILL. (Brief Contributions to Zoology from the Museum of Yale College: No. XLIX.)

AFTER the printing of my last article, in the October number of this Journal, an additional trip to the outer grounds, off Martha's Vineyard, was made by the Fish Hawk, Sept. 21. Owing to unfavorable weather, only two successful hauls (1038, 1039) were made, but some very interesting species were procured. One of the most notable additions to the fauna was a large and perfect sea-urchin, with large spines nearly four inches long (*Dorocidaris papillata*). This had not been taken before on this coast, although not uncommon off the coasts of Europe, and beneath the Gulf Stream, off Florida, etc. The specimen taken at station 1038 measured $7\frac{1}{2}$ inches across the spines. A small comatula (*Antedon Sarsii*) was found at station 1038,

in 146 fathoms, in the greatest profusion, over 10,000 specimens coming up at a single haul. As usual, nearly all the specimens had dismembered themselves before reaching the surface. The great abundance of this and other recent crinoids at certain localities is parallel with the abundance of many ancient fossil crinoids, in particular regions.

In fact, a large number of species, belonging to various zoological groups, in this region are found living gregariously, in vast numbers, at particular spots, while they may not occur at all, or only sparingly, at other stations, in similar depths, and apparently identical in temperature and character of the bottom. Thus, among Echinoderms, the large ophiuran, *Ophioglypha Sarsii*, occurred at stations 918 and 1026, in 45 and 182 fathoms, in vast quantities; at 1026, between two and three barrels (probably over 10,000 specimens) came up in a single haul; the elegant star-fish, *Archaster Agassizii* V., occurred in great numbers at station 997, in 335 fathoms; the more common *A. Americanus* V. has often occurred in very great profusion, many thousands being taken at a haul, at several stations. A slender-armed *Amphiura* occurred in very great numbers at station 920, in 68 fathoms, but was seldom met with elsewhere. Many other echinoderms might also be cited, though affording less conspicuous examples. Several very large actinians, among them *Bolocera Tuediæ*, *Urticina nodosa*, and other species of *Urticina*, occurred in great quantities at many stations (924, 937, 938, 998), more than a barrel of them frequently coming up in the trawl. The pretty bush-like gorgonian coral, *Acanella Normani* V., was very abundant at stations 938, 947, 1029. Of the spiny sea-feather, *Pennatula aculeata*, we took over 500 specimens, at station 1025, and nearly a hundred of *Anthomastus grandiflorus* V., at station 1029; both these forms are usually scarce. The coral, *Flabellum Goodei* V., was abundant at 894, 895, 952, 938. The large and curious annelid, *Hyalinœcia artifex* V., remarkable for the very large, quill-like, free tube that it constructs, must be excessively abundant in many places, as at 869, 880, 1025, 1026, 998, 938, for several thousands are frequently taken at a single haul, and sometimes even four or five bushels, as at station 1032. Among Crustacea, such cases are also very common. A species of *Munida* was very abundant at some stations (871, 922, 941), so that 2,000 or more sometimes came up in one haul, and the same is true of several species of shrimp (*Pontophilus brevirostris* Smith, at 865, 871, 878, 941; *Pandalus leptocerus* S., at 870, 878, etc.); certain hermit-crabs, as *Hemipagurus socialis* S., at 871, 874, 877, 878, 940, 941, 944; the maioid crab, *Euprognatha rastellifera* Stimp., at 871–4, 878, 921, 941, etc.

One of the most striking instances was the occurrence of

a very remarkable and hitherto rare hermit-crab (*Parapagurus pilosimanus* Smith), with its associated, investing polyp (*Epizoanthus paguriphilus* V.)* which is a true commensal, forming, out of its own tissues, the habitation of the crab; and hitherto it has not been found elsewhere than upon the back of this particular species of crab, which, likewise, has not been found without its polyp. Of these associated creatures we took about 400 couples, at station 947, in 312 fathoms, at one haul. It had previously only been known by a few specimens taken by the Gloucester halibut fishermen, in deep water, off Nova Scotia, and by ourselves, in 1880.

In October, while on the way south, Captain Tanner made an independent trip to the edge of the Gulf Stream, off Delaware Bay (stations 1043–1049). On this trip, many interesting species were obtained, among which were several fine large specimens of *Echinus gracilis* and *Dorocidaris papillata*. A large species of *Fissurella*, and a very large specimen of a rare crab, *Geryon quinquedens* Smith, also occurred.

During the summer, the writer made some observations in regard to the phosphorescence of many species. Among those having strong phosphorescence, were *Pennatula aculeata*; *Acanella Normani*; *Urticina nodosa* (in which it is confined to the tentacles and the smoother, soft portion of the column, near the summit); *Ophiocnida olivacea*; *Ophiacantha bidentata*.

Additional dredging stations occupied by the Fish Hawk, in 1881.

Station.	Locality.		Fath.	Bottom.	Date.	Temperature.	
						Bottom.	Surface.
	Off Martha's Vineyard.						
	N. Lat.	W. Long.			1881		
1038	39° 58′	70° 06′	146	sand & shells	Sept. 21	47° F.	67° F.
1039	39 59	70 06	136	"	"	50	67
	Off Delaware Bay.						
	N. Lat.	W. Long.					
1043	38° 39′	73° 11′	130	sand	Oct. 10	49	65½
1044	38 37	73 12	224	gray mud	"	42½	66
1045	38 35	73 13	212	"	"	40	66
1046	38 33	73 18	104	sand	"	51	66
1047	38 31	73 21	156	"	"	49	66
1048	38 29	73 21	435	mud	"	40	66
1049	38 28	73 22	435	"	"	40	66

* *Epizoanthus paguriphilus* Verrill, sp. nov. Polyps few and very large, stout, with broad, swollen bases, arising from a very thick, smooth, lubricous, gray or mud-colored, translucent basal cœnenchyma, which at first invests small univalve shells, occupied by *Parapagurus pilosimanus*, but finally grows far larger than the shell and eventually absorbs it. Disk broad, larger than column; tentacles numerous, rather long, light orange. Breadth of colony, 2 to 3 inches; height of polyps, in expansion, 1 inch or more; diameter, ·5 to ·7 of an inch.

ECHINODERMATA.

Most of the Echinodermata enumerated last year were again taken this season, in still greater abundance, and several additional species were discovered. Among the latter are four species of *Archaster*, one of which is new;* one of the curious round sea-urchins with a flexible test (*Phormosoma Sigsbei*); large specimens of *Dorocidaris papillata*; the European *Echinocyamus pusillus*; three species of Spatangoids (*Spatangus purpureus*, *Brissopsis lyrifera*, both European species, and a *Schizaster*, like *S. canaliferus*); but the last two were also taken in 1880, though not then enumerated.

The *Astrochele Lymani* V., occurred at 939, 1028, 1029, in abundance, twining its arms closely around the branches of *Acanella Normani* V. It had before occurred off Nova Scotia, in the same way, on this and other gorgonian corals. *Ophiomusium Lymani* Thomson was taken several times, in 238–500 fathoms. Several new ophiurans were taken, some of which have not yet been fully examined; and at least one undescribed holothurian.

Schizaster canaliferus L. Agassiz (? variety).

A number of specimens of this singular *Schizaster* have been taken at several stations, in 65–130 fathoms. It is closely related to the Mediterranean form, *S. canaliferus*, and to the species, *S. Orbignyanus*, recently described from the Gulf of Mexico, by Mr. A. Agassiz. Some of the specimens were sent to Mr. Agassiz, who thought them allied to the last named species, but as he had no time to study them carefully, he kindly forwarded specimens of *S. Orbignyanus* to me, for comparison.

My specimens agree exactly with *S. canaliferus*, in form of test, in the shape of the petals, and in the peripetalous fasciole, except that the latter runs directly across the posterior interambulacrum. It differs from both the forms named, in having the lateral fascioles more or less imperfect or abortive on the sides, though distinct below, where they join the subanal fasciole, which is like that of *S. canaliferus*. In some specimens the lateral fascioles, although extremely narrow and faint, can be traced to the peripetalous fasciole, but in many cases it entirely fades out before reaching it. The anal area is usually round, or but slightly elliptical. From *S. Orbignyanus* it differs, also, in having the lower side more convex and the posterior end more swollen and rounded; in having the anterior furrow shorter, broader and deeper; in having shorter antero-lateral petals,

* These species are *A. arcticus* Sars, 183–310 fathoms; *A. tenuispinus* D. and K., 388 fathoms; *A. mirabilis* Perrier (?), 310 fathoms; and *A. Bairdii*, sp. nov., 388 fathoms.

more abruptly bent near their origin; and in having shorter and more ovate posterior petals.

I am, therefore, disposed to consider this a geographical variety of *S. canaliferus*, with which it agrees better than with *S. Orbignyanus*. The latter may also eventually prove to be only a more divergent form of the same species. The variation in the lateral fascioles is a very remarkable one, for this is usually regarded as a generic feature. It approaches *Periaster*.

Brissopsis lyrifera Agassiz.

A large specimen from station 921, after immerson for a short time in alcohol, had the test greenish black above, dark brown beneath, spines dark olive-green. Length, 71^{mm}; breadth, 65; height, 46^{mm}.

Phormosoma Sigsbei A. Agassiz, Bulletin Mus. Com. Zool., viii, p. 75, 1880.

A specimen from station 1029, after being in alcohol a few hours, had the test pale orange-brown above, with a darker orange-brown skin beneath; sutures, tubercles and areas whitish; shell pale salmon beneath the skin. Dorsal spines very slender, few and small toward the center, glassy, delicately fluted and finely spinulose, whitish, faintly tinged with orange; beneath, the spines are larger and longer (12^{mm} and more); tubercles large, with deep areas. Diameter of test, 80^{mm}; height, 18, in the flattened state.

Dorocidaris papillata A. Agassiz.

Our specimens are near the variety *abyssicola*. The long spines appear nearly smooth to the naked eye, but are finely fluted and minutely spinulose; they increase gradually in size for a short distance and then taper very gradually to the truncate or slightly excavated tips; the ventral spines are clavate and truncate, with the distal half strongly sulcated; those near the mouth flat and curved. Color of test and small spines, pale pink; large spines, at base, mostly pale pink above, with three or four broad, faint bands of dull greenish brown; scattered spines (probably reproduced), are dark purplish brown; ambulacral zones and sutures, greenish. Diameter of test of largest specimen, 65^{mm}; height, 45; length of largest spines, 80; their diameter, 6^{mm}.

Archaster Bairdii Verrill, sp. nov.

Disk, broad; arms, broad at base, tapering rapidly to slender tips; the interbrachial spaces have nearly the form of a segment of a circle. Lesser to greater radii, as 1 : 2·20–2·38. The abactinal surface is covered with rather large (1·3–1·6^{mm}), regular, well-

defined paxillæ, those toward the center decidedly larger, each surmounted by a regular rosette of short, bluntly rounded, not very small spinules, which, on the larger paxillæ, form a central cluster of 12 to 20 or more, with a regular circle of slightly larger ones around the edge; these rosettes appear more or less hexagonal, and decrease in size toward the margins and on the arms. The dorsal area of the arms is wide at base, but narrow distally. Marginal plates about 20 on each side of the arms, above and below, rather large, not very convex, evenly covered with rounded granules. Actinal surface with large triangular areas, occupied by regular, rather large, clearly defined paxillæ, with regular rosettes of not very fine, blunt spinules. The adambulacral plates bear, each, a group of about five, rather long, slender, tapering, nearly equal spines, which stand in regular longitudinal rows, the edges of the plates projecting inward but slightly; outside the inner group of spines, there is a rosette of shorter blunt spines, of which the three or four innermost are larger and longer than the outer ones, which are small, like those of the paxillæ. Oral plates not swollen, bordered, on each side by seven or eight, rather stout, vertically flattened spines, and terminated at the inner end by two decidedly longer and stouter ones; their surface bears two regular median rows of seven to nine shorter spinules, and usually a row of three or four small ones between these and the marginal series. Ambulacral feet well developed, with a conspicuous, concave, terminal sucker. Greater radius of one of the largest examples, 38^{mm}; lesser radius, 16^{mm}. Color, when living, light orange.

Station 952, in 388 fathoms. Six specimens.

This handsome species has the form and general appearance of *A. Parelii* and *A. Agassizii*. The latter, of similar size, usually has a rather smaller disk, with longer and less rapidly tapering arms, while the dorsal paxillæ, and especially the central ones, are not one-fourth as large, with much finer and more crowded spinules; the marginal plates often bear a central spine; the ventral paxillæ are more convex, with much finer and more crowded spinules; the adambulacral spines are smaller, finer, and more numerous (8 to 10), with finer spinules outside of them; the oral plates are decidedly swollen, with a crowd of fine spinules over the surface, and with the marginal spines, more numerous, smaller and more acute: it also has large, conical ambulacral feet, with a rudimentary sucker.

A. Parelii agrees nearly with the last in form, but has still longer and more narrowed arms. From *A. Bairdii* it differs in having distinctly smaller, less regular, and more crowded dorsal paxillæ, with more uneven, granule-like spinules; the adambulacral plates project inward over the furrows, and each bears five or six smaller, blunt, rough, marginal spinules in a curved

row, with a group of 10 to 12, or more, smaller, divergent spinules outside of them; the oral plates bear shorter and stouter, round, blunt, marginal spinules; the ambulacral feet have well-developed suckers; the color is dull red, in life.

Ophioglypha aurantiaca Verrill, sp. nov.

Disk large, swollen, nearly round, with small notches, destitute of papillæ, at the bases of the five arms; dorsal surface covered by very numerous, small, imbricated scales, partially concealed by a soft skin; in the central area they are crowdedly arranged around one or more larger central plates; the marginal interradial scales are larger and thicker with a median radial row of two or three still larger ones; ventral scales, convex, unequal, imbricated. Radial shields convex, irregularly subtriangular, with rounded corners and outer edge, as broad as long, separated by a group of three or more imbricated disk-scales. Mouth-shields shorter than broad, with an obtuse inner angle, a nearly straight outer edge, and short, notched, lateral edges. Side mouth-shields long and rather broad oblong, meeting within. Mouth-papillæ very small and irregular, 7 to 9 on each side of each angle, those next to the teeth longer and pointed. Teeth slender, acute. Innermost tentacle-pore large, bordered on the outside by about six small flat scales, on the inner by about four. Arms somewhat rigid, rather short and stout, not seen entire; arm-spines three, the upper one rather long and stout, tapered, the others successively shorter and smaller; two tentacle-scales.

Lower arm-plates rather small, transversely rhomboidal, with rounded lateral angles, the four sides concave, and the distal angle prominent; near the base of the arms the plates are in contact, to a small extent, but farther out they are separated by the lateral plates. Upper arm-plates large, thickened, trapezoidal, toward the base of the arms broader than long, broadest distally, the proximal and distal edges nearly straight: farther out they become longer than broad, and much narrowed proximally. Color, in life, bright orange; in alcohol, white. Diameter of disk, 18^{mm}; its height, 7^{mm}; length of arms (minus tips), from center of disk, 45^{mm}. Off Martha's Vineyard, 192 to 310 fathoms. Specimens of this singular species were sent to Mr. Lyman for examination last year. He considered it an undescribed species. It has no near allies on our coast.

Ophioglypha confragosa Lyman (variety).

This remarkable form was dredged, both this year and last, sparingly, in 238 to 410 fathoms. The identification was made by Mr. Lyman, who was kind enough to compare specimens sent to him, with the type-specimens of his species. His specimens were dredged by the "Challenger," off the La Plata, in

600 fathoms. According to Mr. Lyman, our specimens differ but slightly from the type.

Color, in life, yellowish or grayish white. It is easily distinguished by its rigid arms, with decidedly swollen joints; the conspicuously plated, rigid disk; and by the large, supplementary plate, outside the mouth-shields.

Amphiura macilenta Verrill, sp. nov.

A small species, with very long, slender arms, having three slender, acute arm-spines. Disk, in life, nearly round, often becoming pentagonal in alcohol or when dried; upper surface covered with very numerous, small, rounded, naked, imbricated scales, forming a more or less evident rosette at the center; lower side with more minute scales; radial shields long and narrow, wedge-shaped, the outer ends prominent and in contact, the inner ends separated by a narrow wedge of small scales. Mouth-shields shield-shaped, with rounded corners, rather longer than wide, broadest in the middle, inner angle obtuse; side mouth-shields wide. Mouth-papillæ five on each side of each mouth-angle, unequal in size, mostly obtusely rounded; the innermost one, close to the end of the jaw, is longer and stouter than the rest, obtuse, separated by a slight interval from the next, which is the smallest and most acute; the next two are flattened, the fourth being the broadest; the outermost is small and rounded. Lower arm-plates oblong shield-shaped, longer than broad, with the sides and distal edges nearly straight, and a well-marked proximal angle; toward the base of the arms they are in contact, but farther out are separated by the side arm-plates. Tentacle-scales two, minute, flat; tentacles long and slender. Arm-spines three divergent, nearly equal, a little swollen at base, gradually tapered, acute, about as long as the joints. Upper arm-plates transversely rhomboidal with rounded corners and edges, separated by the side arm-plates, even at the base of the arms. Color, light gray. Diameter of disk, of an average specimen, 4^{mm}; length of arms (minus tips), 60^{mm}.

Very abundant, off Martha's Vineyard, at station 920, in 68 fathoms; also taken sparingly at several other localities.

This has been examined by Mr. Lyman, who does not recognize it as a described species.

SCIENTIFIC INTELLIGENCE.

I. Physics and Chemistry.

1. *On the Vapor-density of the Halogens.*—Victor Meyer has summed up the results of the researches made by himself, by Crafts and by Züblin upon the vapor-density of the halogens. Since Züblin's researches have shown that neither by chemical nor physical means can any different substance be obtained from the vapors of the halogens, it would appear that the diminution of density with rising temperature must be due solely to a dissociation of the molecule into its two atoms. Moreover, this dissociation takes place most readily with iodine and least so with chlorine; a temperature which lessens the density of iodine 50 per cent., causing a diminution of only 16 per cent. in that of chlorine. At high temperatures, therefore, the behavior of the halogens would seem inverted, compared with that at ordinary temperatures. The separation of the atoms from each other in chemical reactions is, under ordinary conditions, most readily effected with chlorine, least so with iodine; while iodine tends strongly to form the molecule I_2, as is seen in the many changes where this halogen is set free. But this abnormality is only apparent; for although hydrogen chloride and ethyl chloride are more permanent bodies than the corresponding iodides, this may very well be due, not to the fact that I has a greater attraction for I than Cl has for Cl, but to the other fact that chlorine has a much stronger attraction for carbon and hydrogen than has iodine. The behavior of the halogens, when heated, then leads to the conclusion, that the attraction of one halogen atom for another similar one follows the same order as its attraction for an atom of any other element. Hence chlorine which is the most active of the three halogens is also the least readily dissociated. Iodine which forms weak molecules with hydrogen, ethyl, etc., forms a weak molecule with iodine. Bromine is intermediate in both directions. The author's experiments have also shown the dissociation of the molecules P_4 and As_4 toward P_2 and As_2. Ferrous chloride vapor, which at low temperatures is Fe_2Cl_4, is $FeCl_2$ at high ones.—*Ber. Berl. Chem. Ges.*, xiv, 1453, July, 1881.

G. F. B.

2. *On an Air-thermometer convenient for Chemical purposes.*—The growing necessity for accurately determining temperatures above 350° C., and the absence of any means of doing this, led Crafts in 1878 to propose a modification of the air-thermometer adapted to such uses. Andrews has now described a still simpler form of this instrument. The thermometer part consists of a capillary tube bent twice at right angles, on one end of which is a bulb of about 1 c. c. capacity and on the other a wider tube about 15 cm. long. At the point where the tube widens is a point of black glass to serve as an index, and in the middle of the wide tube is a glass cock. To the end of this wide tube is attached a

rubber tube with two branches, one of which goes to a closed manometer and the other to a rubber bag. The bag, the manometer and the wide tube are filled with mercury. To make an observation, the rubber bag is compressed till the mercury rises to the black glass index and the height of the mercury column in the manometer is noted. The thermometer bulb is then exposed to the temperature to be measured, the column again adjusted and the new height noted. From these data and the constants of the instrument, the temperature may be calculated. With this apparatus, the boiling point of sulphur was found to be 447°, that of phosphoric sulphide 527°, and that of phenyl phosphate 407°.—*Ber. Berl. Chem. Ges.*, xiv, 2116, Oct., 1881. G. F. B.

3. *On the existence of Phosphorous oxide.*—Reinitzer has examined the product obtained when phosphorus is burned in a limited supply of air, which product is stated in the books to be phosphorous oxide, P_2O_3. This body, when dissolved in water under suitable conditions, affords a gold-yellow solution which coagulates gradually on standing but at once on being heated to 80°, and possesses the properties of Leverrier's solution of his phosphate of phosphorus oxide. A study of this solution showed that the yellow phosphorus compound which it contained was a true colloid in whose molecule the phosphorus and the oxygen exist in the ratio 2 : 3, so that provisionally the formula $(P_2O_3)_m(H_2O)_n$ may be assigned to it. Being a colloid body dialysis furnished a means of obtaining it pure. In this condition, it reacts perfectly neutral, and its dilute solution, like that of other colloids, is not coagulated by boiling, but only by the addition of acids or saline bodies. To the product obtained by combustion, then, the author assigns the formula P_2O_3 or better P_4O_6. But it cannot be considered as the anhydride of phosphorous acid since its aqueous solution reacts neutral and contains a yellow colloid.—*Ber. Berl. Chem. Ges.*, xiv, 1884, September, 1881. G. F. B.

4. *On the Preparation of spontaneously inflammable Hydrogen phosphide.*—Brössler has re-examined H. Davy's statement that the action of zinc upon dilute sulphuric acid in presence of granulated phosphorus, produces spontaneously inflammable phosphine, Dumas in 1826 having asserted that the gas evolved was hydrogen, mixed, when the solution was heated, with phosphorus vapor. Brössler confirms Davy's assertion and finds that when some pieces of common phosphorus are placed in a mixture of dilute sulphuric acid and zinc, a regular evolution of gas takes place forming bubbles on the surface which break and ignite spontaneously, giving a brilliant light and a white smoke. The reaction is most satisfactory when the liquid has a temperature of 40° to 50° C., though after it has once commenced it continues even at the temperature of 20°. The gas evolved is readily absorbed by solution of copper sulphate. The author finds the same result to take place when potassium hydrate solution acts on zinc and phosphorus at 60° C. The hydrogen phosphide evolved by the action of hydrochloric acid upon tin in presence of phosphorus is the ordinary variety

not spontaneously inflammable. But upon the addition of a few drops of nitric acid to the mixture the gas inflames spontaneously. —*Ber. Berl. Chem. Ges.*, xiv, 1757, Sept., 1881. G. F. B.

5. *On Ammonium tribromide.*—Roozeboom finds that when to a saturated solution of ammonium bromide, sufficient bromine is added to form H_4NBr_3, and the whole is placed over sulphuric acid for a few days, large prismatic crystals are formed, having the color of potassium dichromate. The heat of their formation is considerable. When a solution of 9.8 grams ammonium bromide in 13·93 grams water is mixed with 8·39 grams bromine, the temperature rises from 20° to 28·6° C. At ordinary temperatures the crystals lose the excess of bromine in from one to two hours; at 50°, in ten minutes. They are not hygroscopic and dissolve readily in water. By drying the crystals in an atmosphere of bromine, they were prepared for analysis. This was effected: 1st, by estimating the loss of weight at 100°; and 2d, by titrating the iodine set free when a weighed portion of the tribromide was put into a solution of potassium iodide. The results confirmed the formula H_4NBr_3. No dibromide appeared to be formed, but from the continued solution of the bromine with additional evolution of heat, the author infers the existence of a pentabromide, H_4NBr_5.—*Ber. Berl. Chem. Ges.*, xiv, 2398, Nov., 1881. G. F. B.

6. *On the Petroleum of the Caucasus.*—Beilstein and Kurbatow have continued their examination of the petroleum found in the Caucasus and have now given the results of their investigation of the oil from Zarskije Kolodzy, in Tiflis. The wells there belong to Siemens & Halske of Berlin, from whom the samples examined were obtained. This petroleum is much lighter than that from Baku and yields a larger quantity of volatile fractions. Three of these were at first obtained, boiling respectively between 30°–35°, 70°–75° and 95°–100°. The first fraction consisted of pentane, and was a mixture of nearly equal parts of normal pentane and isopentane. From the second fraction hexane was isolated; but though the boiling point was correct, analysis showed a deficiency of hydrogen and the specific gravity was too high. Suspecting the presence of the addition-products of the aromatic series corresponding to the formula C_nH_{2n}, the liquid was treated with bromine and with concentrated nitric acid in order to destroy them. The suspicion was confirmed by the fact that after this treatment the liquid possessed all the properties of normal hexane. The third fraction consisted of heptane, from which, by treatment with fuming sulphuric acid, traces of benzene and toluene were obtained. It would therefore appear that the petroleum from Central Caucasus, unlike that from the Caspian Sea, consists essentially of the hydrocarbons C_nH_{2n+2}, like American petroleum, but contains small quantities of the aromatic hydrocarbons C_nH_{2n-6} and their addition-products C_nH_{2n}. Treatment of the crude petroleum with nitric acid of 1·52 sp. gr. gave volatile crystals, which were obtained in relatively greater quantity from the fraction boiling

between 40° and 50°. Recrystallized from alcohol, they appeared as brilliant broad needles, fusing at 95°–96°, insoluble in water but easily soluble in boiling alcohol, in ether, petroleum naphtha, carbon disulphide, etc. On analysis they gave the formula $C_4H_8(NO_2)_2$ which is that of dinitrobutane.—*Ber. Berl. Chem. Ges.*, xiv, 1620, July, 1881. G. F. B.

7. *On the reactions of Chinoline.*—The antiseptic and antipyretic properties of chinoline having led to its extensive introduction into commerce, DONATH has given the following reactions of this base, by which its presence may be recognized: (1) Chinoline salts in aqueous solution are precipitated milky-white by alkali-hydrates. The precipitate is difficultly soluble in excess, but readily soluble in ether, benzene, alcohol, less readily in CS_2, chloroform and amyl alcohol. Sodium carbonate precipitates the solution white with evolution of carbon dioxide. (2) Ammonia precipitates chinoline salts, white, the precipitates being pretty easily soluble in excess. (3) Iodine in potassium iodide solution (7 parts KI, 5 parts I in 100 H_2O) produces a red-brown precipitate, insoluble in hydrochloric acid. Delicacy 1 : 25000. (4) Phosphomolybdic acid (10 parts sodium phosphomolybdate in 100 water made strongly acid with nitric acid) gives with a chinoline solution acidulated with nitric or hydrochloric acid, a yellowish white precipitate easily soluble in ammonia. Delicacy 1 : 25000. (5) Picric acid (1 part to 100 water) gives a yellow amorphous precipitate, soluble in alcohol, difficultly in hydrochloric acid, easily with a reddish-yellow color in potassium hydrate. Delicacy 1 : 17000. (6) Mercuric chloride (5 parts to 100 water) gives a white flocculent precipitate, readily settling, soluble in hydrochloric acid, difficultly in acetic acid. Separated from dilute solutions in crystalline needles. Delicacy 1 : 5000. (7) Potassium-mercuric iodide (5 parts KI, 1·4 parts $HgCl_2$ and 100 parts H_2O) gives a yellowish-white amorphous precipitate, which on the addition of hydrochloric acid, is converted into slender amber-yellow needles. This reaction is characteristic and its delicacy is 1 : 3500. (8) Potassium ferrocyanide produces a yellow color, and on the addition of a mineral acid, a reddish-yellow amorphous precipitate falls which soon becomes crystalline. Delicacy 1 : 1000. (9) Potassium dichromate, carefully added, forms fine dendritic crystals soluble in excess of the reagent.—*Ber. Berl. Chem. Ges.*, xiv, 1769, Sept. 1881. G. F. B.

8. *On the theory of the Peptones.*—POEHL has experimentally confirmed a theory of the formation of peptones proposed in 1873 by Eichwald; i. e., that liquid albumen in contact with animal tissues at the temperature of the body is converted into peptone. This he has done not only by repeating Eichwald's experiments, but by extending the statement to albuminates in various states, even coagulated. He believes that all albuminates are converted into peptone by contact with animal and plant tissues. The formation of peptone for example, when lung or kidney tissues act at blood-heat on swollen fibrin takes place as energetically as the action of pepsin. The view of Eichwald that between the

true albuminates and the peptones various transition forms exist, depending simply on various states of hydration, the author has also confirmed by finding that peptone by treatment with dehydrating agents, as alcohol and alkali-salts, is reconverted into precipitable albumen.—*Ber. Berl. Chem. Ges.*, xiv, 1355, June, 1881. G. F. B.

9. *On the existence of Allantoin in Vegetables.*—The young shoots and leaves of trees contain, as is well known, asparagin which comes probably from the splitting up of albuminates. Believing that other nitrogenous bodies ought to be simultaneously formed, SCHULZE and BARBIERI have examined the freshly opened buds of the plane tree (*Platanus orientalis*) for these substances and have found a body apparently identical with allantoin. The young shoots, dried, were extracted with hot water, the extract decomposed with lead acetate, the filtrate freed from lead and evaporated. In 12 to 24 hours, crystals of the new body separated, mixed with asparagin. The latter may be separated by fractional crystallization or by converting it into the comparatively insoluble copper compound. The filtrate from this after removing the copper by H_2S, yields small, brilliant crystals, difficultly soluble in cold water, and giving the well-known mercury and silver reactions of allantoin. Analysis gave the formula $C_6H_4N_4O_3$. The occurrence of this uric acid derivative in the vegetable kingdom is highly interesting.—*Ber. Berl. Chem. Ges.*, xiv, 1602, July, 1881. G. F. B.

10. *Observations upon Klangfarbe.*—Dr. Rudolph König discusses the question of the origin of harmonic tones and partial tones and investigates the connection between *Klangfarbe* and the change of phase of the pendulum movements involved in the production of sound waves. He does not find that tuning forks kept in motion by electrical means and provided with resonators give true or concordant results, and has accordingly constructed a form of siren by means of which the influence of the form of pendulum movement upon the color-tone can be studied without the disturbing influence of resonators. Different forms of sinusoidal curves representing pendulum movements in various phases are cut upon the edge of thin iron rings which are arranged in a conical form upon an axle. These rings can be rotated in front of various blast pipes from a wind bellows and the blast pipes can be directed either upon the summit of the waves or between the elevations. Discussions of the results afforded by this apparatus are given.—*Ann. der Physik und Chemie*, No. 11, p. 369. J. T.

11. *W. Siemens upon the Dynamo-Electric Machine.*—This paper is in a certain sense a reclamation. The history of the development of the dynamo-electric machine is given as far as it relates to the author's investigations. The development of heat in the machine is considered, and it is maintained that the so-called Foucault currents are not the only cause of the heat which is developed in rotating iron armatures. When these armatures are divided into thin plates separated by insulating media, in order to break up the currents of induction, the iron was still very

much heated. This heating is attributed to the sudden reversals of magnetism when the magnetization approaches the maximum of the magnetic capacity of the iron. In the transmission of power by electricity one-half of the useful effect is consumed in the development of heat, and in the development of magnetic effect where it can not be practically utilized. This loss is a serious drawback to the future of the electrical transmission of power. The following considerations should be observed in the construction of dynamo-electric machines:

(1.) All disposition of conductors which does not conduce to electro-motor effects should be avoided.

(2.) The conductivity of all wires employed to produce such effects should be as great as possible.

(3.) Metals should be arranged so as to avoid Foucault currents.

(4.) The magnetism of the electro-magnets should be fully utilized in the most direct manner.

(5.) The division of the windings of the inducing wires, through which currents of changing direction flow, should be as small as possible; the number of divisions should be made as large as possible to avoid the extra currents which arise with each change of current.

The Hefner von Alteneck machine and the Gramme machine are then discussed from the point of view of the above considerations. The advantages of a new construction of Siemens' armature, in the so-called "Topfmaschine" is dwelt upon, and some measures of the effects of a machine based upon the principle of Unipolar Induction are given.—*Ann. der Physik und Chemie*, No. 11, 1881, pp. 469–483. J. T.

12. *Theory of the rotation of the plane of Polarization of Light.*—Both the ordinary and the magnetic rotation of the plane of polarization of light, E. Lommel considers can be fully explained by the mathematical discussion of the following hypotheses:

(1.) The ether contained between the molecules of bodies has the same properties as the free ether.

(2.) Between the ether and the particles of the body an effect can be produced analogous to those produced by friction, which is proportional to their relative velocities.

(3.) Each particle is affected by an elastic force peculiar to it.

(4.) Each particle is influenced by a resistance proportional to its velocity.

Electrical currents circulating about the molecules must be influenced by the above retarding causes. The mathematical discussion, based upon the above hypotheses, leads to results which agree, on the whole, very well with Verdet's observations on the change of the plane of polarization of light in bisulphide of carbon and in kreosote. The author considers that the divergence of results obtained by Maxwell's formula from the observed results in kreosote, proves its untrustworthiness. Lommel's theory also explains the non-magnetic rotation of the plane of polarization.—*Ann. der Physik und Chemie*, No. 11, 1881, pp. 523–534. J. T.

13. *On the Electrical Resistance of a Vacuum.* — Professor Edlund, of Stockholm, in a memoir presented to the Swedish Academy in April, 1881, has reviewed the results of the long series of experiments made by various physicists during the past century, and bearing upon the question as to the degree of resistance which a vacuum opposes to the passage of an electrical current. His conclusions differ materially from those which have been ordinarily accepted. That, as has been shown by different experimenters, electricity cannot pass through a nearly perfect vacuum, is not questioned, but this is regarded to be due not to a sudden increase in resistance of the vacuum itself, but to the resistance of the passage of the electricity from the electrodes to the surrounding medium. Representing by r_1 the specific resistance in a column of gas of unit length, and by r the resistance to the passage from the electrode to the medium, then the total resistance for a tube of length l will be $r+r_1 l$. Professor Edlund argues that experiments prove that as the medium is rarefied the tension of the electricity needed for a discharge diminishes up to a certain limit (depending upon the distance between the electrodes, the width of the tube, surface of the negative electrodes, and so on), after which it must increase. As r_1, for theoretical reasons, diminishes as the rarefaction is increased, this fact just stated can be explained only by assuming that r increases with the rarefaction; for ordinary pressures r is very small compared with r_1 and can be neglected, and then it follows that the tension needed for a discharge is proportional to the distance. As the pressure is diminished r increases and r_1 decreases, and then the tension necessary for a discharge increases with the distance between the electrodes but in a smaller ratio. When the rarefaction is great, r_1 can be neglected as compared with r, and then the tension is independent of the distance. Professor Edlund states various observations confirmatory of these conclusions and concludes finally that "a vacuum is a conductor of electricity," and, further, that the assumption that the presence of ordinary matter is necessary for the passage of the electrical current cannot be maintained.

14. *An Elementary Treatise on Electricity;* by JAMES CLERK-MAXWELL, M.A., edited by WILLIAM GARNETT, M.A. 208 pp. with 6 plates. Oxford, 1881 (Clarendon Press).—The larger portion of the present volume was written by the late Professor Maxwell, some seven years ago, and a part of it was used by him in his lectures on electricity at Cambridge. Unfortunately, other labors caused the author to defer the completion of the work, and at the time of his death only about one-half was entirely finished. The manuscript of a portion of the remainder of the volume, as originally planned, was partially completed, while some important subjects were yet untouched. The volume, as now presented to the public, is, however, far from giving the impression of incompleteness which might be inferred from the above statements, for the editor with wise discretion has filled the vacant

places by selections from Professor Maxwell's well-known larger work on *Electricity and Magnetism*. That this is necessarily accompanied by a loss of strict homogeneity and an occasional sacrifice in the logical order of treatment of individual topics is obvious, but while this makes us regret anew that the author's labors were so prematurely interrupted, it does not seriously affect the value of the book nor prevent it from accomplishing the end for which it was planned. The author's design, as is stated in the fragment of the preface which he left for us, is to "present in as compact a form as possible those phenomena which appear to throw light on the theory of electricity, and to use them each in its proper place for the development of electrical ideas in the mind of the reader." There are many conscientious students of electricity who are unable to master the mathematical methods of Professor Maxwell's larger treatise and yet who desire to obtain an accurate knowledge of the fundamental theories of the subject; to them the present volume, in which mathematical analysis is in general avoided, will be invaluable. In fact it would be impossible for any one, however far advanced, not to gain new ideas or to see old ones in a clearer light, after a careful study of Professor Maxwell's lucid exposition of the subject.

15. *Tables of Qualitative Analysis;* arranged by H. G. MADAN, M.A., F.C.S. 20 pp. 4to. Oxford, 1881 (Clarendon Press).—These tables present a synopsis in an unusually convenient form of the steps through which a student must go in examining qualitatively any ordinary substance, simple or compound. The instructions given under the successive heads are necessarily very brief, so that the student must either already have considerable knowledge or else receive constant assistance from an instructor. A proper use of such tables is very useful in systematizing the knowledge of the student, but it is possible for him to use them too mechanically.

II. GEOLOGY AND MINERALOGY.

1. *International Geological Congress at Bologna.*—The sessions of the International Congress began at Bologna on the 26th of September and continued for one week. The following results of the Congress are taken from a report of the Secretary, M. G. DEWALQUE, of Belgium, published in the Proceedings of the Geological Society of Belgium.

With regard to geological maps, it was decided that a chart of Europe should be published, on 49 sheets, the scale 1: 1,500,000. The chart is to be executed at Berlin under the direction of MM. BEYRICH and HAUCHECORNE, assisted by an international committee of four members from other countries, M. MOSISJOVICS for Austria, M. DAUBRÉE for France, Mr. TOPLEY for Great Britain, M. GIORDANO for Italy, and M. DE MÖLLER for Russia, with M. RENEVIER, of Switzerland, as Secretary.

The subject of the nomenclature for the series of geological formations was also considered and the following conclusion reached.

That for the highest division the term Group be used; that the term for the next lower range of subdivisions be System; for the third, Series (Section in French, Abtheilung in German); for the fourth, Stage; that for the lowest, Assise or Couche in French. Formation was not adopted for either of the higher subdivisions because it is so often used with reference to mode of origin, and also for rocks of a kind without reference to age; and Terrain, because it could not be introduced into the German and Russian languages. By the above, the science will have a Paleozoic *group*, a Silurian *system*, a Trenton *series*, and so on.

The corresponding terms having reference to time in geological history that were recommended by the Congress are, in descending order, Era, Period, Epoch, Age. The position of the subdivision *Age* is not well chosen. In English if it is desired to express the idea of indefinite time, we say the *ages* of the past—periods having limits, and epochs being strictly times of the occurrence of particular events; and it is hence hardly practicable to apply the word Age to a subdivision of an Epoch in Geology.

It was recommended that the scale for a chart of Central Europe be 1 : 500,000. It was proposed also that in the coloring of geological maps the colors which shall be used on the geological chart of Europe be those as nearly as possible that shall hereafter be generally adopted. Rose-carmine will have the preference for the crystalline schists unless shown to be of Cambrian or later age; violet for the Triassic, blue for the Jurassic, green for the Cretaceous, and yellow will be reserved for the Cenozoic group. The notation in letters on the map will be based on the Latin alphabet; the letters used will be the initials (capital) of the name of the system; and an additional letter or numeral for subdivisions of the system, the numbering starting from the oldest included subdivision. For eruptive rocks the same rule will be followed, except that the Greek letter will be employed in place of the Latin.

The directors of the International chart of Europe will meet at the extraordinary session of the Geological Society of France in 1882, and that of the Helvetic Society of Natural Sciences in 1883, in order to explain the condition of their work; and the two Committees of the International Congress will also be present.

Some general rules also with regard to nomenclature were adopted: one that priority beyond the 12th edition of Linnæus's Systema shall not have authority; and another—which will be questioned by many, though good in purpose and in its consequences—that, in future, priority for specific names shall not be irrevocably acquired unless the species shall have been not only described *but also figured.*

The third meeting of the Congress will be held in Berlin in 1884.

2. *Notice of the discovery of a Pœcilopod in the Utica slate formation.*—Through the courtesy of my friend, Rev. William N. Cleveland, of Holland Patent, Oneida County, New York, I

am able to call attention to the discovery of the remains of a large *Pecilopod* in the Utica slate, north of that village. The remains consist of a large endognathary arm of seven or nine joints, provided with long backward curving sabre-like spines, and a portion of a thoracic somite, probably one-half of the ventral surface of the anterior somite. For this species I propose the name *Eurypterus? Clevelandi*, n. sp., and will give a detailed description with figures of the specimens, in a future number of the Journal. C. D. WALCOTT.

New York, Jan. 10, 1882.

3. *Coal-Field near Cañon City, Colorado.*—Professor J. J. STEVENSON has described, with detailed sections, this valuable coal-field in a paper published in the Proceedings of the American Philosophical Society for October 7, 1881 (p. 505). It occurs in a small area of Laramie rocks lying along the eastern foot of the Greenhorn Mountains. In a generalized section of the beds thirteen coal beds, from six inches to six feet in thickness (numbered A to M), alternate with shale and sandstone. At the Cañon City Coal Company's shaft, at Rockvale, there are coal beds, A to H, in a thickness of 341½ feet. The sandstone above No. H at some places contains the fucoid *Halymenites major* in great abundance; and this fossil occurs in other localities in the sandstone of the same horizon, and occasionally also at the base of the series; the thickness of the beds between the upper and lower limits is over 400 feet. Professor Stevenson states that he had before found the same fossil abundantly (this Journ., III, xvii, 370), along with dicotyledonous leaves, in a sandstone shown by other fossils to be of the Fox Hills group, and that he had also found the fucoid in a sandstone, 60 to 80 feet thick, of the Trinidad coal-field of Southern Colorado and Northern New Mexico, but not below or above this sandstone; and he had recognized the bed as marking the base of the Laramie group. The sandstone occasionally holds a coal bed, and at one place contained a *Cardium*-like mollusk, too imperfect for determination.

4. *The Paleolithic Implements of the Valley of the Delaware.* 25 pp. 8vo. Proc. Boston Soc. Nat. Hist. for Jan. 19, 1881.—This pamphlet is a collection of short papers on the Delaware Valley paleolithic implements, severally by Messrs. C. C. ABBOTT, the discoverer of them, H. W. HAYNES, G. F. WRIGHT, LUCIEN CARR and M. E. WADSWORTH, with concluding remarks by F. W. PUTNAM. Mr. Putnam closes his remarks respecting these very interesting discoveries and his own visit to the locality, as follows:

"As Dr. Abbott has stated, in his historical summary of the discovery of the implements in the gravel, it has been my good fortune to take, with my own hands, five unquestionable palæolithic implements from the gravel at various depths and at different points. The relation of the circumstances under which one of these was found will be sufficient to convince you that the implement was in the position where it was buried by the four feet of gravel which had been deposited over it.

A short distance from Dr. Abbott's house and very near where the Trenton gravel joins the marine gravel, there is a deep gully through which flows a small brook. In this gully the gravel bank is constantly washing away and presenting new surface exposures. After a heavy rain in June, 1879, I visited the spot with Dr. Abbott and his son. Here I noticed a small boulder of about six or eight inches in diameter, projecting an inch or two from the face of the bank, about four feet from the surface of the soil above; I worked the stone from the gravel in which it was firmly imbedded and drew it out. At the back part of the cavity thus made I noticed the pointed end of a stone, and after working it up and down a few times, so as to loosen the gravel about it, I drew out the implement now exhibited.

On the same day I discovered a second specimen in place, eight feet from the surface, and Dr. Abbott's son Richard found another about four feet from the surface. These three specimens were found within twenty or thirty feet of each other, after a heavy shower had made the most favorable conditions for their discovery. A long continued search, on several following days, at various places along the gravel bluff, failed of success in finding other specimens in place, although several were obtained from the talus. This shows how seldom the implements are likely to be found, and it may be from this cause that some unsuccessful hunters have doubted the occurrence of the implements in the gravel. Certainly the evidence that has been brought forward to-night will clear away all doubts as to the importance and reliability of Dr. Abbott's discoveries and investigations, which have proved the former existence of paleolithic man in the valley of the Delaware."

5. *Bryozoans of the Upper Helderberg Group;* by JAMES HALL. 36 pp. 8vo. Read before the Albany Institute, March 29, 1881. Albany, 1881.—This paper is an abstract of the section on Bryozoans of the Upper Helderberg and Hamilton beds in the Thirty-third Report upon the State Museum of Natural History, made in January, 1880, which has not yet been published.

6. *Tertiary of the Eastern and Southern United States.*—ANGELO HEILPRIN has several important articles on the Tertiary of Eastern North America and its fossil mollusks in the Proceedings of the Academy of Natural Sciences of Philadelphia for the years 1880 and 1881.

7. *Cambarus primævus, a Cray fish, from the Lower Tertiary beds of Western Wyoming*; by A. S. PACKARD, Jr., with a plate. Bull. U. S. Geol. and Geogr. Survey under F. V. Hayden. Vol. vi, No. 2.

8. *American Museum of Natural History* (Central Park, New York). Bulletin No. 1.—This first number of the Bulletin of the American Museum contains three handsomely illustrated and valuable papers, by the paleontologist of the Museum, Mr. WHITFIELD—On a new Crinoid from Burlington, Iowa; On Dictyophyton and new allied forms from the Keokuk beds; and Observa-

tions on the purposes of the embryonic Sheaths of Endoceras, and their bearing on the origin of the siphon in the Orthocerata.

9. *Petroleum in the Northwest Territories of British America, on the Athabasca and elsewhere.*—A paper on this subject, by Dr. R. Bell, is contained in the Proceedings of the Canadian Institute, new Ser., i, 225, 1882.

10. *Traité de Géologie par A. de Lapparent*, Ancien Ingénieur au Corps des Mines, Professor à l'Institut Catholique de Paris. Paris (F. Savy).—This treatise on geology, by M. A. de Lapparent, the associate of Delesse in the preparation of his annual Revue de Géologie, is now in course of publication, only 480 pages having thus far reached this country. This portion, making fascicules 1 to 3, is occupied with the physiographic and dynamical parts of the science. The author has laid out a broad scheme, and so far as completed, the work promises to be quite full and thorough in its treatment of its subjects. After a general introduction, it takes up "Terrestrial Morphology" under the heads of morphology proper and physiography, in which are presented many details from terrestrial physics. Book Second has the title "External Terrestrial Dynamics," and extends to page 366; and Book Third, that of Internal Terrestrial Dynamics. The last treats of the earth's interior temperature, volcanoes, and related topics, and is not finished with the close of fascicule 3. The author has drawn his illustrations for his excellent treatise largely from his own country, yet ranges widely also for illustrations of the subjects.

11. *Jelly-like Hydrocarbon from Scranton, Pennsylvania.*—The jelly-like, carbonaceous mineral, resembling dopplerite, the occurrence of which in a peat bog at Scranton, Pennsylvania, was described by Mr. T. Cooper, (see this Journal, December, 1881, p. 489), has been further investigated by Professor H. C. Lewis, of Philadelphia. The substance, as previously stated, forms thin veins in the muck at the bottom of the peat bog. It is black in color, and when first taken out it is jelly-like in consistency. On exposure to the air it becomes tougher and elastic, somewhat like India-rubber. When in this condition a thin slice can be cut by a knife; it is then seen to be brownish-red by transmitted light, and nearly homogeneous. When completely air-dried it is brittle and nearly as hard as coal and resembles jet, having a brilliant resinous luster and conchoidal fracture; its specific gravity is 1·032. Before drying it burns slowly in a Bunsen burner and without flame, but when dry it burns with a clear, yellow flame. It dissolves partly in hot alcohol, but wholly in caustic potash, even in the cold. An analysis by Mr. J. M. Stinson, of the Pennsylvania Geological Survey, of material dried at 100° C., gave:

C	H	N	O	Ash
28·99	5·17	2·46	56·98	6·40=100·00

Analysis of the dry separated material gave—

Volatile matter,	72·19
Fixed carbon,	21·41
Ash,	6·40
	100·00

The empirical formula calculated is $C_{10}H_{22}O_{16}$, which requires C 30·15, H 5·53, O+N 64·32=100.

In physical characters this substance very closely resembles the dopplerite of Haidinger, from Aussee (see Dana's Min., p. 749), but is different in composition. Similar jelly-like hydrocarbons have been described by others. Lewis suggests that these jelly-like hydrocarbons may be grouped together under the name *phytocollite* (=plant-jelly), but the new name seems very unnecessary.—*Am. Phil. Soc. Philadelphia*, Dec. 2, 1881.

12. *Brief notices of some recently described minerals.*—BERGAMASKITE is a variety of amphibole remarkable as containing no magnesia. It occurs in a quartzose hornblende-porphyry from Monte Altimo, Province of Bergamo, Italy. Forms acicular crystals vertically striated, with prismatic cleavage at an angle of 124°. G.=3·075. An analysis gave: -($\frac{0}{2}$) SiO_2 36·78, Al_2O_3 15·13, Fe_2O_3 14·46, FeO 22·89, CaO 5·14, MgO 0·93, Na_2O 4·00, K_2O 0·42, loss 0·25=100.—LUCCHETTI in Z. Kryst., vi, 199.

LITIDIONITE.—E. Scacchi describes blue lapilli from Vesuvius, consisting of a white earthy substance with a glassy-blue crust. An analysis gave: SiO_2 71·57, CuO 6·49, FeO 4·02, K_2O 10·92, Na_2O 6·78=99·78. Fuses easily; the nucleus, consisting of augite, olivine, etc, is infusible. The author on the ground of the fusibility regards the substance as a mixture of quartz and the carbonates of potassium and sodium. Obviously the name given does not apply to any definite mineral species.—*Rend. Accad. Napoli*, Dec., 1880.

NOCERINE.—Announced by A. Scacchi as occurring in volcanic bombs in the tufa of Nocera. Found in white acicular crystals referred to the rhombohedral system. In composition regarded as a double fluoride of calcium and magnesium. Associated with fluorite, some brown crystals referred to amphibole, and minute hexagonal crystals perhaps microsommite. The exterior of the bombs is covered with mica.—*Accad. Linc. Trans.*, III, v, 1881.

MELANOTEKITE.—Massive, cleavable in two directions. H.=6·5. G.=5·73. Luster metallic to resinous. Color, black to blackish-gray. Opaque. Analysis gave: SiO_2 17·32, Fe_2O_3 23·18, PbO 55·26, CuO 0·20, FeO 0·75, MnO 0·69, CaO 0·02, MgO 0·59, K_2O 0·24, Na_2O 0·54, BaO 0·11 (?), Cl 0·14, P_2O_5 0·07, ign. 0·93=100·04. The calculated formula is $Pb_2Fe_2Si_2O_9$. Described by G. Lindström as occurring with native lead, magnetite and yellow garnet at Longban, in Wermland, Sweden. This locality has previously furnished two other silicates of lead—hyalotekite and ganomalite.—*Öfv. Ak. Förh. Stockholm*, XXXV, 6, p. 53.

13. *Artificial Enstatite and Chrysolite.*—M. St. Meunier has recently succeeded in forming artificially a variety of minerals by

exposing the metal, a silicate of which is to be formed, when at a red heat, to the vapor of water and that of silicon chloride. This method is regarded by the author as of especial interest as imitating the conditions realized in the natural formation of the primitive minerals of which we have examples in various types of meteorites. He has, for example, succeeded in producing enstatite, a common mineral in meteorites, and that in forms which it often there assumes, as, for example, in clusters of needles radiating from a center, showing in a section the characteristic fan-shaped or star-shaped forms. It is in similar forms in meteorites that the imagination of Mr. Otto Hahn has found crinoids and sponges. M. Meunier remarks, with some humor, that it is significant that these supposed fossils can be formed in a porcelain tube heated to redness. By varying the conditions the author has also succeeded in obtaining a pulverulent mixture of chrysolite (peridote), with the enstatite, and he adds that were this powder, together with a nickeliferous iron, cemented together, we should have a rock essentially identical with that of natural meteorites.—*C. R.*, xciii, 737.

14. *Supposed Organic Remains in Meteorites.*—A year since Mr. Otto Hahn published in Germany a work purporting to describe a large series of fossil sponges, crinoids, corals, and so on, identified by the author in some stony meteorites, more especially that of Knyahinya, Hungary. These conclusions are so fanciful and so obviously without foundation in fact, that it would be unnecessary to refer to them except that a recent article has been published in this country in which the discoveries are described in the spirit of the author, and which is calculated to mislead those unfamiliar with the subject. The supposed organic forms seen in thin sections of the meteorite are merely the radiated and stellated forms which the mineral enstatite often assumes in meteorites, and which M. Meunier has shown (see above), can be imitated successfully. They bear the same relation to fossil remains that dendrites of manganese oxide bear to vegetable impressions. Professor J. Lawrence Smith, who has done more than any one else to make us acquainted with the various carbon compounds which occur in meteorites, has not long since expressed himself (*Courier Journal* for Nov. 22, 1881, Louisville), very strongly as to the observations of Mr. Hahn. Among other remarks he says: "Were these remains present we should discern carbonate of lime on their interior. The two or three that have any carbonate of lime were discovered and analyzed by myself, and in these cases the carbonate of lime was an accidental constituent of incrustation deposited on the surface after their fall."

It is only simple justice to Mr. Hahn to say that while his conclusions will not be accepted, his memoir is a most valuable contribution to the study of the structure of meteorites. The photographic plates which accompany it are remarkably beautiful and can be studied with profit. It is only the author's interpretation of them which is at fault.

15. *Tabellarische Uebersicht der Mineralien nach ihren krystallographisch-chemischen Beziehungen geordnet von* PAUL GROTH.—Zweite vollständig neu bearbeitete Auflage. 134 pp. 4to. Braunschweig, 1882. (Fr. Vieweg & Son). — Professor Groth's volume, while modest in its plan, is still a valuable contribution to mineralogy, more especially on the chemical side. It furnishes a list of all well established mineral species, classified according to chemical characters and crystalline form, giving for each its chemical formula, crystalline system, and axial relations. It is intended particularly for the use of students, but will be useful for all who desire to have before them a classification of minerals in clear and concise form. In reviewing the chemical composition of many species the author has often been able to make important suggestions as to the way in which the formula should be written, and in this way he introduces a large number of original ideas which throw light on doubtful points. The relations of the different isomorphous groups of compounds has never been better presented.

III. BOTANY AND ZOOLOGY.

1. *Natural History Nomenclature.*—The Zoological Society of France has issued a pamphlet entitled "Règles applicable à la Nomenclature des Êtres Organisés," which it proposes for adoption, and upon which comments are requested. The rules were to be submitted to a Geological Congress held last autumn at Bologna.

The rules are brief and simple, and occupy less than three pages. A Report from a committee, by M. Chaper, fills the remainder of the 37 pages. The rules being for both kingdoms of organized beings the opinion of botanists is invited. These rules, proposed in the interest of Zoology, are in such agreement with those practiced in Botany, and especially with those in the digest made by M. DeCandolle a few years ago, that they call for little remark, and that mostly in small particulars. In some cases a sensible recommendation or a preferable rule has, perhaps, been pushed to extremity. For instance, specific names of essential identity of meaning and derivation, but of different form, such as "*fluviorum*, *fluvialis* and *fluviatilis*" are not allowed in the same genus. The introduction of such names may well be objected to, but if two of them had found their way into a large genus of plants, botanists would hardly regard it as a case of double employment of the same name.

Specific names from persons are required always to be nouns in the genitive. Botanists have always used the adjective form more or less, and we see no reason why they should forego it. *Lamarckianus* is as good as *Lamarckii* and better than *Lamarcki*, to which these zoologists would restrict us. For their rule goes on to require that all such words (excepting those which have been actually used and declined otherwise in the Latin language) shall

make their genitive in a single *i*. We think that we have seen this rule applied in some zoological works, and with cacophonous results. Surely the ear must judge whether a barbarous name shall be latinized in *us* or *ius*. In many cases the choice may be nearly indifferent; in others by no means so. Who would write *Pecki* or *Becki* in place of *Peckii* or *Beckii*? Fortunately the laws before us direct that such specific names shall be written with either a small or a capital initial according to the rule generally followed in writing. So we need not follow the custom which in England certain zoologists have forced upon one botanical work, and other zoologists are adopting in this country, which would require us to write *pecki*. It is strange that any one could wish to have a mechanical and senseless uniformity override all other considerations.

By one rule, the name of an author when appended to that of a species must always be printed in a different type from that of the species. As the omission of this causes no confusion in writing, it might seldom cause any in print. But ordinarily the reference, of which this name is the abbreviation, is in different type from the name which precedes. It is hardly a matter to prescribe by law.

That the names of families should always end in *idæ* may be practicable in zoology, but hardly so the rule against homonyms, viz: that the name of a genus which has been reduced to a synonym shall never again be used as such in the same kingdom, nor a specific name so reduced be ever used again in the genus. It is laid down, also, that no name once published shall ever be discarded on account of its impropriety. That surely depends on the degree of impropriety.

The *Exposé des Motifs* in the Report by M. Chaper sometimes takes questionable ground; in one place it seems to contradict the rule last referred to, declaring that the name *Novæ-Hollandiæ* must be changed to *Neo-Batavus*, first, because that is the proper Latin form, and second because all specific names from countries should be adjectives,—which is quite as unnecessary as their other rule that all names from persons shall be genitives.

The Report brings out a point which the rules do not pronounce upon, viz: that the zoologists take the ground that the name of a species transferred by a later writer to some other genus shall still be followed by the name of the original founder, thus making species override genus in importance, and confusing chronology. We have so fully and repeatedly expressed our opinion on the matter in this Journal that we need not farther refer to it here. The last adopted laws of botanical nomenclature and the practice of almost all phænogamous botanists agree in rejecting this innovation. The reduction of the rank of genus by zoologists to the last degree of analysis gives some excuse for the practice, and the practice seems likely to facilitate the degradation.

Naturally we have indicated only points in which the proposed rules do not harmonize with the received botanical code. A. G.

2. *Maximowicz, de Coriaria, Ilice et Monochasma*, etc. A quarto paper in the Mém. Acad. St. Petersb., 1881. pp. 70, and four plates.—A full discussion and monograph of *Coriaria*, and the conclusion reached that this monotypical genus should stand next to the *Simarubaceæ*. *Ilex* is similarly discussed as to the character and extent of the genus, is divided into four sections in a somewhat new way, seemingly with excellent judgment; but *I. Cassine* falls to one section and *I. Dahoon* and *I. myrtifolia* to another. *Monochasma* is a new Chinese genus near to *Bungea*, and to *Schwalbea* is assigned a character for better distinguishing it from *Siphonostegia*, viz: that the placentæ become free from the valves before the maturity of the capsule. A. G.

3. *Ueber sogenannte Compasspflanzen.* By Professor E. STAHL. —In this paper, which is an extract from the *Jen. Zeitschrift*, Stahl gives the result of his experiments with *Lactuca Scariola* and *Silphium laciniatum*, for the purpose of ascertaining the conditions which cause the leaves of the plants named to assume a meridional position. In the case of the *Silphium*, which is the common Compass-plant of the Western States, the fact that the leaves point in a northerly and southerly direction has long been known, but in the case of *Lactuca Scariola*, although it had been observed that the leaves were often vertical, Stahl was the first to notice that they generally stood in meridional plane. In both plants, the peculiar position of the leaves is best seen when they grow in unsheltered places, exposed to bright sunlight; while when crowded together, or growing in the shade, the leaves generally assume the common horizontal position. The leaves of *Lactuca* are arranged on the stem in the $\frac{2}{5}$ order. Those on the north side of the stem become vertical by a twisting of the petiole, the upper surface of the leaf facing the east. Those on the south side by a similar twisting become vertical with the upper surface facing the west. The leaves on the east and west side of the stem do not exhibit any torsion of the petioles, but they become upright with their upper surfaces approximated to the stem. Stahl took two plants growing in pots and placed one where it would be exposed to direct sunlight from 10 o'clock until 3, and kept in the dark for the rest of the day; the other was placed so that from sunrise until 10 o'clock and from 3 o'clock until sunset it was exposed to the sunlight, but from 10 to 3 was in the dark. In the first case the leaves did not assume a meridional position, but in the second case they did. That the meridional position is produced by the sun when near the horizon is clearly shown by the following experiment. A pot with several young plants was placed in a window facing the north, where the plants received direct sunlight a few hours after sunrise and before sunset. In this experiment the leaves bent toward the north with their upper surfaces turning either to the east or to the west. The pot was then placed farther back in the room, so that the plants were not exposed to the direct sunlight, and the leaves then assumed a position at right angles to the diffuse light from

the window. Stahl concludes that the meridional position of the leaves of *Lactuca Scariola* is due to the common diaheliotropism observed in most leaves, and that these leaves differ from those of other plants only in their greater sensitiveness to intense light. In *Silphium* there is a torsion of the petioles as in *Lactuca*; and, if the petioles are fastened so that they cannot bend, the blade of the leaf itself twists. Stahl states that a meridional position of the leaves can be seen clearly in *Aplopappus rubiginosus* and to some extent also in *Lactuca saligna* and *Chondrella juncea*, and he believes that many other examples will be found, especially among the plants of dry and exposed regions. W. G. F.

In a number of the Botanical Magazine, published during the past year, in which the American Compass-plant is illustrated, it is recorded that I noticed, during the previous year, in a plant growing in the Botanic Garden of Harvard University, one leaf in which there was an abrupt torsion of 90° in the middle of the blade. I cannot now say whether or not the basal portion was free to change its direction. A. G.

4. *The Brain of the Cat*, Felis domestica, by BURT G. WILDER, Proc. Amer. Phil. Soc., 1881.—An elaborate memoir occupying 40 pages and illustrated by 4 fine lithographic plates.

IV. ASTRONOMY.

1. *Notation of Comets.*—The Council of the *Astronomische Gesellschaft*, at the recent meeting at Strassburg, recommended that the notation of comets in any year by the letters *a*, *b*, *c*, etc., according to the date of discovery, be given up altogether. There is good reason for the recommendation. The notation I, II, III, etc., according to perihelion passage, is that which it is important to retain after the year has passed. Two notations so similar tend to confusion. Until the order of the perihelion passage is determined the comets can be readily distinguished by the names of their discoverers, adding the date of discovery if necessary, so that the notation by letters is superfluous. H. A. N.

2. *Astronomische Nachrichten, begründet von H. C. Schumacher.*—This veteran Journal has just completed its hundredth volume. Dr. A. Krueger, who has succeeded the late Dr. Peters as Director of the Observatory at Kiel, has also succeeded him in the editorship of the Journal. By an understanding with the Prussian Government the Council of the *Astronomische Gesellschaft* promises assistance and countenance in order that the *Astronomische Nachrichten* shall be the central and leading organ of Astronomical periodical literature. H. A. N.

3. *Washington Observations for* 1877—*Appendix III.*—Professor EASTMAN gives us in this appendix a value of the solar parallax from meridian observations of Mars. In September, 1876, Admiral Davis invited the coöperation of other observatories in making, after a plan prescribed by Professor Eastman, meridian observations upon Mars at the opposition of 1877, for

the purpose of determining the solar parallax. Returns which could be used in the investigation were received from Melbourne, Sydney and the Cape of Good Hope in the Southern, and from Cambridge (Mass.) and Leyden in the Northern Hemisphere.

After the discussion and comparison of the observations, rejecting a very few that are palpably erroneous, Professor Eastman says: "Taking the mean of the remaining *seventy* results from all the stations, with regard to the weights, we have—

$$\pi = 8''{\cdot}980 \pm 0''{\cdot}0172.$$

The results from the Melbourne and Cambridge combination seem to indicate either that the value found for π [$9''{\cdot}1382$] is certainly too great, or that the observations at the first five stations are all affected by a systematic error.

This difference may arise from the method of observing over inclined threads at Cambridge, for the agreement of the results among themselves is very satisfactory; but, whatever the cause of the discrepancy may be, it has not been deemed advisable to employ these values in obtaining the final result.

Omitting the results derived from the Cambridge observations, and also those inclosed in parentheses, and taking the mean of the remaining *sixty* results with regard to the computed weights, we have for a final result—

$$\pi = 8''{\cdot}953 \pm 0''{\cdot}019.$$

This value of π is undoubtedly greater than would be assigned by a large majority of astronomers, but it fairly represents what the method will give from such observations as were at hand for this discussion." H. A. N.

4. *Thirty-sixth Annual Report of the Director of the Astronomical Observatory of Harvard College;* by EDWARD C. PICKERING. 16 pp. 8vo. Cambridge, 1882.—Professor Pickering's Annual Report gives a gratifying statement of the present condition of the Harvard Observatory, and of the work which has been accomplished there during the past year. The brief summary of the work done includes: (1) a large number of photometric observations of the eclipses of Jupiter's satellites; (2) observations of stars having singular spectra—the spectra of all the stars north of $-40°$ marked as red or colored in the Uranometria Argentina have been examined in the large telescope, and from this and also some miscellaneous sweeping, a list of about eighty stars having banded spectra has been published; (3) photometric study of variable stars; (4) observations of the comets of 1881; (5) observations with the meridian circle, and (6) with the meridian photometer. The distribution of time signals has been continued under the charge of Mr. F. Waldo until June 1, and since then of Mr. Edmunds; it is stated that the error is believed to rarely exceed two-tenths of a second in clear weather and four-tenths when cloudy, and that the error seldom changes more than two-tenths of a second from one day to the next.

Among the investigations proposed for the coming year are the measurement of the position of the principal lines in the spectra of all banded stars as yet known; also the photometric determination of the brightness of various points on the surface of the moon; the measurement of the light of faint stars, for which a system of standards of brightness has been devised with the coöperation of other astronomers. The new meridian photometer will be used for measuring variable stars and their comparison stars, and also for determining the light of the brighter asteroids.

V. Miscellaneous Scientific Intelligence.

1. *Mining Industries: Paris Exposition*, 1878; by A. D. Hague, Commissioner.—This valuable review of the Mining Industries of the various nations of the globe contains a condensed survey of mining products, mining processes and materials, mining imports and exports, mining regions, ores and ore-deposits, coal fields, diamond fields, etc., consumption of metals, coals, etc.

2. *Were Ancient Copper implements hammered or moulded into shape.* — Professor F. W. Putnam, of Cambridge, Mass., closes a note on this subject in the Kansas City Review for December, with the following statement:

"That copper was used in large quantities by the Indians there is no doubt, and it was also used to a considerable degree by the tribes who erected the burial mounds in the Ohio valley and throughout the southwest, whoever they were; but I have not yet seen a single object made of copper from these sources that I should regard as having been cast; on the contrary, the evidence of hammering, and rolling between stones, is more or less clearly shown in all by the character of the surface and by the distinct lamination of the metal in places, when carefully examined with a lens."

3. *United States Coast Survey.*—Mr. J. E. Hilgard, Chief Assistant in the Coast Survey Department, has recently been appointed Superintendent in place of Captain C. P. Patterson, deceased. Mr. Hilgard is eminently fitted for the position.

3. *Beebe's Four-Place Tables.*—This book contains, in clear type and compact form, the logarithms of numbers with proportional parts, and the trigonometric functions, both logarithmic and natural, at intervals of ten minutes, with their differences. The arrangement is the same as that of Gauss's five- and Vega's seven-place tables, and is therefore the best possible. This feature makes them an excellent introduction to the larger works. A very careful comparison with the standard tables reveals no mistakes. Published by H. H. Peck, New Haven.

Sensation and Pain, by Charles Fayette Taylor, M.D. 78 pp. 16mo. New York, 1881. (G. P. Putnam's Sons).

Opium Smoking in America and China, by H. H. Kane, M.D. 156 pp. 16mo. New York, 1882. (G. P. Putnam's Sons).

Greenwich Spectroscopic and Photographic Results. 84 pp. 4to. 1880.

Bulletin of the Minnesota Academy of Natural Sciences, Minneapolis, Minn. Vol. ii, No. 3, 1881. pp. 37–238.

OBITUARY.

JOHN WILLIAM DRAPER, M.D., LL.D.—The death of Dr. Draper, which occurred on the 4th of January, removes the most renowned investigator in molecular physics and the most encyclopedic author in the circle of American scientists. Although born in England, and trained there in youth, Dr. Draper's life-work and field of labor were in America. Born at St. Helens near Liverpool, May 5, 1811, he had his early education in a Wesleyan school at Woodhouse Grove, where his scientific tastes were developed. His studies in chemistry and physics were afterward pursued under a private teacher, and later at the London University. Following the greater part of his family, he came to this country in 1833, at the age of 22, and in 1836, took the medical degree from the University of Pennsylvania where his thesis on the Crystallization of Camphor under the influence of light, and kindred subjects, was selected by the Faculty for publication. This is the first of a long series of experimental researches on radiant energy. It led to his appointment, in the same year, to the chemical and physiological chair in Hampden Sidney College, Virginia, where he had the opportunity to prosecute other investigations in the same line. These, without effort on his part, brought him, soon after, the appointment to the Professorship of Chemistry and Physiology in the University of New York, where he remained through the entire period of his life. In 1841 he was active in establishing the University Medical College; and he resigned the duties of presiding officer of the Medical Faculty only in 1876. For more than forty years, Dr. Draper was quietly engaged in careful experimental researches in physiology and molecular chemistry, publishing the results of his work in this Journal, the Journal of the Franklin Institute, the American Journal of Medical Science, and in the London Philosophical Magazine. These researches cover a very large range of subjects, but were mostly devoted to a study of the chemical phenomena of light, in both the organic and inorganic world. In 1878 he collected and condensed these researches and published them in a separate volume under the title of "Scientific Memoirs, being Experimental Contributions to a Knowledge of Radiant Energy" (473 pp. 8vo). This volume was noticed at the time of its publication in vol. xvi, p. 390 (1878) of this Journal. It remains a noble monument to his memory, made of the results of labors which have greatly advanced the sum of human knowledge.

Of the substance of the thirty separate memoirs which constitute it the author says: "Among many other subjects treated of in these pages the reader will find an investigation of the temperature at which bodies become red hot, the nature of the light they emit at different degrees, the connection between their condition as to vibration and their heat. It is shown that ignited solids yield a spectrum that is continuous, not interrupted. This has become one of the fundamental facts in astronomical spectroscopy. At the time of the publication of this memoir (1847), no one in

America had given attention to the spectroscope, and, except Fraunhofer, few in Europe. I showed that the fixed lines might be photographed, doubled their number, and found other new ones at the red end of the spectrum. The facts thus discovered I applied in an investigation of the nature of flame and the condition of the sun's surface. I showed that under certain circumstances rays antagonize each other in their chemical effect, and that the diffraction spectrum has great advantages over the prismatic, which is necessarily distorted. I attempted to ascertain the distribution of heat in the diffraction spectrum, and pointed out the great advantages if wave-lengths are used in the description of photographic phenomena. I published steel engravings of that spectrum so arranged. I made an investigation of phosphorescence and obtained phosphorescent pictures of the moon. Up to this time it had been supposed that the great natural phenomena of the decomposition of carbonic acid by plants was accomplished by the violet rays of light; but by performing that decomposition in the spectrum itself, I showed that it is effected by the yellow. Under very favorable circumstances I examined the experiments said to prove that light can produce magnetism, and found that they had led to an incorrect conclusion. The first photographic portrait from the life was made by me. . . . . I also obtained the first photograph of the moon. . . . . When Daguerre's process was published, I gave it a critical examination, and described the analogies existing between the phenomena of the chemical radiations and those of heat. For the purpose of obtaining more accurate results in these various inquiries I invented the chlor-hydrogen photometer, and examined the modifications that chlorine undergoes in its allotropic states. Since in such researches more delicate thermometers are required than our ordinary ones, I entered on an investigation of the electromotive power of heat, and described improved forms of electric thermometers. In these memoirs will be found a description of the method made use of for obtaining photographs of microscopic objects, together with specimens of the results. In a physiological digression respecting interstitial movements of substances, I examined the passage of gases through thin films such as soap-bubbles, and the force with which these movements are accomplished, applying the facts so gathered to an explanation of the circulation of the sap in plants, and of the blood in animals. Returning to an inquiry as to the distribution of heat and of chemical force in the spectrum I was led to conclude, in opposition to the current opinion, that all the colored spaces were equally warm; and that, so far from one portion—the violet—being distinguished by producing chemical effects, every ray can accomplish special changes. This series of experiments, on radiations, is concluded in this volume by an examination of the chemical action of burning-lenses and mirrors."

This is a remarkable series of claims for one investigator to set up on a single line of research. But they were well founded; and

this was substantially affirmed by the report of the Committee of the American Academy of Arts and Sciences, at Boston, which, after an impartial review of Dr. Draper's work, recommended the award by the Academy of the Rumford Medals for the year 1875 (one of gold and one of silver) to Dr. Draper for his "Researches on Radiant Energy."* This was only the sixth award of the Rumford Medal in about sixty years. The previous five awards were (1) to HARE (1839) for the invention of the oxy-hydrogen blowpipe and improvements in voltaic apparatus; (2) to ERICSSON (1862) for his caloric engine; (3) to TREADWELL (1865) for improvements in management of heat; (4) to ALVAN CLARK (1867) for improvements in the lenses of refracting telescopes; (5) to CORLISS (1870) for improvements in the steam engine. This enumeration shows that the award to Dr. Draper was for important *discoveries* in science, while the others, excepting in the case of Dr. Hare, were for *inventions* and *improvements*. It rarely falls to the lot of any one investigator in physical research to add so largely to the triumphs of science over the unknown.

Our admiration, however, of the author's ability and industry is increased, when we review the considerable number of important memoirs which he has contributed to the departments of chemistry, electricity and physiology. We have to regret that the author did not find strength, in his declining years, to give to the world his memoirs on chemical, electrical and physiological topics, some of them hitherto unpublished, of which he says, in the preface to the volume mentioned: "These, for the present, I must reserve." But it is a great satisfaction that he has left, as his literary executors, sons whose names are well known in the annals of science, from whom we may look for the collection of memoirs so reserved. A full list of Dr. Draper's papers, corrected by himself, will be found in the writer's address entitled "Contributions to Chemistry," 1874, pages 78–82.

But Dr. Draper was not simply an experimental investigator of natural phenomena. He was an earnest and also a deep thinker in the department of the philosophy of history and human progress, and his works in this direction were among his most elaborate efforts. He says: "From the study of individual man it is but a step to the consideration of him in his social relations, and this, accordingly, had been done in the second part of my work on Physiology. But the subject being too extensive to be dealt with satisfactorily in that manner, I have published the materials that I had collected in a separate book under the title of 'A History of the Intellectual Development of Europe.'" The view which he aimed to illustrate in this work is that the intellectual progress of nations proceeds in the same course as the intellectual development of the individual; that the movement of both is not fortuitous, but under the dominion of law. His "Thoughts on the Civil Polity of America," his elaborate "History of the American Civil War," in three volumes, and his "History of the Con-

* Proceedings American Academy, II, xi, 313, 325, May 25, 1875, March 8, 1876.

flict of Religion and Science" are all logical sequences of his view of human progress. He remarks, "When I thus look back on the objects that have occupied my attention, I recognize how they have been inter-connected, each preparing the way for its successor. Is it not true that, for every person, the course of life is along the line of least resistance, and that in this the movement of humanity is like the movement of material bodies?" As many may have regarded Dr. Draper as a materialist, and at least as indifferent if not hostile to religion, it is a satisfaction to affirm that he was a theist and a firm believer in a future state of existence for which the present is only a preparation.

For many years Dr. Draper has dwelt in a quiet retreat at Hastings on the Hudson, a few miles from New York City, near the astronomical observatory of his son, Dr. Henry Draper, the well-known astronomer, and surrounded by everything that could minister to the tastes of a veteran in science. Here he quietly surrendered, on the morning of January 4, 1882, a life crowned with the rewards of a long devotion to his favorite pursuits, and honored by the diffusion of his fame through the translation of his more elaborate works into a great number of European and Asiatic languages. His wife died many years before him, leaving three sons and three daughters. All the sons have adopted pursuits of science, kindred to the father's taste, and have made names for themselves now well known in the walks of science.

The funeral of Dr. Draper was attended, January 10th, by a throng of mourners, including delegates of numerous scientific societies with which the deceased had been connected—the Faculties of Medicine and Arts, Science and Law, of the University of New York, and members of the Faculties of the Universities of Vermont and Pennsylvania. The religious ceremonials were observed in St. Mark's Church, in New York, where the remains were brought from the village church at Hastings on the Hudson, and finally found their resting-place in the Greenwood Cemetery, on Long Island. B. S.

Lewis H. Morgan, eminent in American Ethnology and Archæology, died on Saturday, the 14th of December last, at Rochester, New York. He early began his study in his favorite department among the Indians then remaining in Western New York. His published works and memoirs are all the result of extensive research, and gave him the leading place among American archæologists. The most important are "The League of the Iroquois" (1851), "The earliest Systems of Consanguinity and Affinity of the Human Family," published by the Smithsonian Institution in a quarto volume of 700 pages (1873), "Ancient Society, or Researches in the line of human progress from Savagery through Barbarism, into Civilization" (1877), "Houses and House-life of the American Aborigines" (1881); and in another line, the "American Beaver and his Works" (1867). Mr. Morgan was President of the American Association at the meeting in 1880 at Boston.